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RECENT PROGRESS IN ENGINEERING.

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On the Progress of Machinery.

By DANIEL KINNEAR CLARK, M. Inst. C. E.

The Progress of Public Works Engineering.

By L. F. VERNON-HARCOURT, M. Inst. C. E.

In the department of mechanical engineering, steam machinery occupies the first place in magnitude and importance; and the records of research afford evidence of the assiduity and success with which foreign engineers and philosophers have prosecuted their investigations into the generation and performance of steam as a motive power. First in order comes M. G. A. Hirn, who has, during upwards of twenty years, occupied himself with experimental inquiries into the action and behavior of steam in the steam engine, in which he has been ably assisted by his associates. He accurately appreciates the powerful and almost instantaneous operation of the walls of the steam cylinder, and their action on the steam, for every individual stroke of the piston. A portion of the steam, when cut-off for expansion, during the remainder of the stroke, is condensed as it enters the cylinder, and is, to a greater or less extent, reconverted into steam, towards the end of the expansion and during the exhaust. This action and re-action are augmented as the period of expansion is increased; and the loss of efficiency in one direction overbalances the gain of work in the other, when steam is worked expansively beyond given moderate limits.

M. Mallet gives an interesting historical summary of experimental investigation, reaching back to the year 1856, into the existence of such alternate condensation and re-evaporation of steam in the cylinder.

M. Hirn has reduced the process of test experiments on steam engines to a system remarkable for precision, and for the success with which the mechanical equivalent of heat is employed as a factor. He employs a thermo-dynamic formula, in which two series of quantities are to be equated. On the one side, there is the whole of the heat supplied to the engine, comprising the heat of the steam, taken as dry, passing into the cylinder, the heat conveyed by primed water, and the heat given up by the steam condensed in the jacket. On the other side, there is the heat converted into work, the heat lost by radiation from the jacket, and the heat delivered to the condenser. M. O. Hallauer applies this formula to the results of many trials of Woolf engines and single cylinder engines; and the valuations agree, within from ½ to 1 per cent. of error—affording powerful evi-

dence of the reality as well as the utility of the mechanical theory of heat.

M. Hallauer contributed the results of observations and experiments towards a solution of the question of clearance-space, in relation to the compression of exhaust steam, and its bearing on the efficiency of steam in the engine. He argued, from the evidence of indicator-diagrams, in the case of an ordinary Woolf engine, that there was a gain of 10 per cent. of efficiency by the compression of the exhaust steam, as against the absence of compression; a deduction which he was enabled subsequently to corroborate by direct proof, in the case of a Woolf engine, of which the valves were so modified as to augment the period of compression in the first cylinder, from $\frac{1}{10}$ of the stroke before alteration to $\frac{1}{4}$. The economy effected by this alteration amounted to from $3\frac{1}{2}$ to $6\frac{1}{2}$ per cent. What the radical difference of efficiency may be, due to conditions of clearance and non-clearance, is not finally settled. M. Hallauer maintains, after all, that a single-cylinder beam-engine, with four valves (Corliss fashion, in which the clearance space is minimised) and steam-jacketed, may be at least as economical as a Woolf engine or a receiver engine. The results of this experiment go to prove that for compound engines, the actual ratio of the total expansion should not exceed from 1 to 5 to 1 to 7.

M. Farcot, following up the principle of the Corliss type, constructed a direct-acting pumping engine, in which the valves of the steam-cylinder were so closely adjusted that the total clearance at each end of the cylinder was not 1 per cent. of the working capacity of the cylinder. The cylinder was jacketed with steam direct from the boiler; the fuel consumed was found, by careful trials, not to exceed 1.543 lb. of coal per indicator HP. per hour. Supposing that the water had been evaporated at the rate of 8 lbs. per lb. of coal, this consumption would correspond to 12.34 lbs. of water per indicator HP. per hour;—a performance probably unparalleled. It must be added, nevertheless, that a beam Woolf engine in regular work at Ghent, tested by M. Dwelshauvers, was proved to be capable of developing $391\frac{1}{3}$ indicator HP., with a consumption of only 14.63 lbs. of water per indicator HP. per hour, and of coal 1.94 lb. Also, a pair of compound rotative beam pumping engines for the waterworks at Lawrence, Mass., when tested for performance, consumed just 14 lbs. of water per indicator HP. These performances—one of a Corliss engine, the others of Woolf engines—may be accepted as amongst the best of their kinds, and they may be taken to show, in corroboration of M. Hallauer's conclusion, that no decided pre-eminence, in point of efficiency, can be claimed for one type of engine over the other.

It is further made manifest from the experiments of M. Hirn and his associates, that steam may be worked at least as efficiently in a single cylinder without a steam jacket, when superheated, as when worked in its ordinary state of saturation, either in a Woolf engine or in a single cylinder steam-jacketed. This is a valuable practical conclusion, which, though it has been vaguely anticipated, has not hitherto, in England, been brought to the test of direct comparison.

It is indicated by the results of experiments made by Mr. Isherwood, that a very moderate degree of super-heating suffices to secure the economy that may be effected by the substitution of superheated steam for ordinary steam in the cylinder.

Mr. Charles E. Emery reports the results of experiments with the U. S. steamers, the "Bache," the "Rush," the "Dexter," and the "Dallas," in which the performances of single cylinders and compound cylinders were compared, and the action of the steam-jacket was tested. With the "Bache," a reduction of consumption of water from 11 to 12 per cent. by the addition of the steam-jacket was effected, whilst the tendency of the evidence was to show that the compounded cylinders were more economical than the second cylinder taken alone. But the superiority of the compound system was, perhaps, more clearly put in evidence by the results of the trials with the other steamers. Comparing the "Rush," having compound steam-jacketed cylinders, with the "Dexter," having a single cylinder, felted and lagged, steam of about 80 lbs. total pressure having been employed in both cases, the most economical ratio of actual

expansion in the "Rush" was 1 to 6¼, whilst in the "Dexter" it was only 1 to 3½; and, in the "Rush," the consumption of steam was 23 per cent. less than in the "Dexter." This was the total economy, and, from the evidence with the "Bache," it may be estimated that half of this, or 12 per cent., was due to the action of the steam-jacket, and the remainder, 11 per cent., to the benefit of the division of expansive work between two cylinders. These inferences must be taken as limited to cylinders having the proportions of those on board the individual steamers, and they do not invalidate the general conclusion already drawn from the results of continental experience.

The variation of the internal resistance of steam engines with the whole work done, has been experimentally elucidated by M. Walther-Meunier, who tested a horizontal Woolf engine, with bedded cylinders, at the mills of MM. Dollfus, Mieg & Co. When the engine developed, in the cylinders, 95.5, 131. 3, and 191.3 indicator HP. successively, the power of the brake was 81.9, 86.3, and 89.1 per cent. of the indicator power respectively. It therefore appeared that the internal resistance of the engine rose slowly from 17.3 to 17.9 and 20.9 indicator HP.; and that in doubling the total indicator power the internal resistance was augmented by 3.6 indicator P. only. These data may serve to correct some erroneous assumptions with respect to the rate of augmentation of internal resistance, which, in this instance, is unexpectedly low, and which at full power did not exceed 11 per cent of the total indicator power.

The value of the mechanical equivalent of heat continues to be a subject of inquiry. M. G. A. Hirn tested it by four independent series of investigations:—of the friction of water, of the flow of water under high pressures, of the crushing of lead under a ram, and of the fall of temperature due to the expansion of air. On the evidence, taken together, M. Hirn concluded that the most nearly correct value of the equivalent was 432 Killogrammeters per calorie, or, in English measures, 787-4 foot-pounds per heat-unit. At the same time, he retained for use the value 425 kilogrammeters (or 774.70 foot-pounds), which has been adopted by most continental authors on the theory of the mechanical equivalent of heat. It may be stated, in this connection, that the Committee of the British Association for determining the value of the mechanical equivalent have reported that 772.55 foot-pounds, subject to some small correction, is the equivalent at the sea level. This value is confirmatory of the old unit, 772 foot-pounds. Nevertheless, M. Violle deduces from the results of many experiments made by him with a gyroscope revolving between the poles of an electro-magnet, the quantity, 790 foot-pounds, as the true mechanical equivalent: a value which differs little from that deduced by M. Hirn.

M. Paul Havrez endeavored to reduce to a geometrical ratio the law of the decreasing evaporative performance of the heating surface of steam boilers, as it recedes from the furnace. His deductions were based on the results of experiments by Mr. C. W. Williams and Mr. John Graham, and the more recent experiments of M. Petiet and others in France, on locomotive boilers. M. Petiet's experiments showed, that the surface of the firebox—exposed to radiated heat from the fire—evaporated per unit of surface by far the largest proportion of water, and that the efficiency of the boiler surface was rapidly reduced, advancing towards the smokebox, insomuch that the evaporation was diminished by one-half for each interval from yard to yard; that is to say, each lineal yard of tubes evaporated only half the quantity of water per hour that was evaporated by the preceding yard.

M. Paul Havrez also formulated the results of a long series of observations made by himself and his brother, with the object of determining the best proportions of boilers. He developed many novel and interesting relations between power, surface, capability, and weight of boilers of different classes, which, whether they are to be taken as final or as provisional, throw much light on some of the hitherto occult properties, with respect to efficiency, of steam boilers of various types. In the second part of his investigations, M. Havrez, led, to some extent probably, by the results of his prior investigations on the efficiency of evaporating surface, advocated a species

of boiler which may be called, "united boilers" (*Chaudieres accolees*) in which he employed, to the largest possible extent, direct heating surface over a spacious furnace, with a large and extended central flue, surrounded by boilers and heaters.

The comparative merits of Lancashire boilers and French or elephant boilers, formed the subject of a course of experiments instituted by the "Societe Industrielle de Mulhouse." The results of the experiments showed a remarkable identity of performance of the two boilers, both as to evaporative performance and as to efficiency of fuel; and they tended to prove that plate heating-surface would do the same duty for evaporation, whether presented in one form or in another. The reporters concluded that, regarding economy of result, the design of the boiler was of little consequence, and that the truly important point was to provide large heat-absorbing surface in relation to the coal burned. The second part of the conclusion was in accordance with the continental taste for extensively developed heating surface, without taking into consideration the area of the firegrate.

It has been shown by the results of comparative experiments made by Prof. Thurston, that wet-tan fuel can be successfully burned under steam boilers. It was proved that, for the best action, the fuel required to be dried under the cover of firebrick ovens, the heat retained, and the combustion completed, before the gaseous products touched the surface of the boiler; and that, for the same object—the retention of the heat—the grate should be of firebrick. A boiler and furnace constructed according to these provisions, evaporated 1.74 lb. of water per lb. of wet tan; whilst in other boilers, where the same fuel was deposited on ordinary cast-iron grates, and burned in contact with the boiler surface, only 1.16 lb. of water was evaporated per lb. of wet tan.

Similar treatment has been found beneficial for the combustion and utilization of powdered fuel under steam boilers. In the experiments made by Mr. Isherwood with powdered coal, the powdered fuel was mixed with a blast of air, and burned under a brick arch turned over the firebars. The general bearing of the experimental results showed that coal, whether anthracite or bituminous, when pulverized for combustion, was decidedly less efficient for evaporation than the same fuel in lumps. But the inferiority here displayed probably resulted from depriving the boiler of the action of the radiant heat of combustion, by the interposition of the enveloping arch, which was removed when the lump coal was burned.

The value of a blast of compressed air for augmenting the evaporative power of steam boilers has been satisfactorily established by M. L, E. Bertin, who estimated that the quantity of steam consumed for compressing a supply of air to act as a blast in the chimneys of steam boilers amounted to only one-tenth of the quantity required when discharged as a blast; direct into the chimney. The power developed by the engine of the steamer "Resolve" was more than doubled by the application of the compressed air-blast to the boilers, whilst the power required for the generation of the blast did not exceed 5 per cent. of the power of the engine.

In America, a spiral exhaust-nozzle has been introduced by Mr. Thomas Shaw, by the use of which the noise of steam blowing-off at the safety-valves of locotives and steam vessels is effectually quelled.

The causes of corrosion and incrustation of steam boilers have been perseveringly investigated by French engineers. M. Hétet explains that the deposits which take place in steam boilers, worked with surface condensers, consist of iron-oxide and acid fats, forming ferruginous soaps containing 50 per cent of iron, and that the iron is obtained by internal corrosion of the boiler. He employed a chalk mixture to neutralize the acid, and to arrested the corrosion of the boiler and the formation of deposits there. M. Allaire contends for the substitution of refined neutral oils, which are chemically harmless, and are totally consumed in the cylinder without suffering decomposition or generating deposits. M. Stapfer also proposes the use of mineral or neutral oils for lubricating, although he notices that a stiff gum-like substance may be formed, analagous to caoutchouc, which adheres to the piston-rod.

M. Audenet suggests that a species of

trap should be placed at the entrance from the exhaust pipe to the condenser, for the interception of greasy impurities. He estimates, from the results of sobservations made on the surface-condensers of the "Dives," that the quantity of heat which passed through tube-surfaces when new, amounted to 500 units per square foot per hour per degree Fahr. difference of temperature. But he recognises that this value must be diminished by dedosits of fatty matter, and he alludes to the consequent loss of vacuum in transatlantic steamers fitted with surface-condensers, amounting to from 1·20 to 2·40

For the prevention of incrustation, zinc and talc have each been used in steam boilers. The effective action in each case is mechanical in its nature. With zinc, according to M. Lesueur, a decomposition of water takes place, by which the zinc is oxidised, whilst free hydrogen gas is discharged from the surface of the iron plates of the boiler, and baffles, in a greater or less degree, the deposition of scale on the surface. It does not appear that much is to be hoped for from the zinc process; but the talc process promises better. Pulverized talc has been employed successfully in locomotive boilers on the Paris and Lyons Railway, and, whilst it prevents incrustation, it has also the effect of reducing the friction of the pistons and the piston-rods. Its action appears to be purely mechanical; in virtue of its scaly structure, it presents a great extent of surface, offering many points of attachment for the salts of incrustation.

MM. Beranger and Stingl have arranged a process of preliminary purification of the feed-water of boilers, in which the inorganic elements of impurity are precipitated by chemical treatment, and the water is decanted and filtered prior to its being delivered to the boiler.

The theory of furnace chimneys has been discussed by continental writers. Whilst Signor P. Guzzi assumes that the draught increases with the temperature at all temperatures, M. Cornut, who is probably in the right, maintains that the draught, though it increases at first with the temperature, diminishes with the higher temperature, and that it reaches a maximum when the temperature amounts to 560° Fahr., taking the external air at 50° Fahr. Mr. Robert Briggs gives a useful formula for the sectional area of chimneys for steam-boilers.

In the practice of locomotives, attention continues to be directed to the development of full-power engines. Herr Steinberg in conjunction with Herr Schau, designed and employed an eight-wheeled coupled engine for the traffic of the Poto-Tiflis Railway. The wheel-base was limited to 10 feet 8 inches, when employing wheels of only one meter in diameter. Radial axles were not employed, but the leading and the trailing axles were simply free to move laterally.

In a posthumous memoir, by M. Le Chatelier he recommends the employment of two engines coupled together for mountain traffic, but he omits to point out how they should be connected, so that they may not baffle each other. For the satisfactory solution of the problem of full-power enginea, flexibility of base, combined with radial axles mvst be recognized as necessary elements. The principle of the double bogie, already already worked out in England by Mr. R. F. Fairlie, has been applied by M. Rarchaert in a locomotive, which is placed on two four wheeled bogies. The wheels of the bogies are coupled, and the bogies are also coupled together by a central coupling-rod under the boiler, to which the power of a single pair of cylinkers is communiceted through a transverse crank-shaft. Compared with an ordinary six-coupled-wheel engine, the relatively slight wear of the wheel flanges attested the greater freedom of the Rar. chaert engine on the rails, It also employs the whole of the weight for adhesion, with one pair of steam-cylinders

The central-rail locomotive for mountain traffic, as described by M. Desbriére, has been gradually improved; although it remains a question to be settled by experience, whether two cylinders or four cylinders should be employed. In tqe engines of this kind, constructed for the Cantagallo Railway, Brazil, by the introduction of a few obvious and simple modifications, the breakage of the machinery, frequent in engines of an earlier class, have disappeared; whilst in other engines, for New Zealand, the dispersion of oil from the interior machinery upon the surfaces of the central rail, has been entirely prevented by the application of a shield.

To diminish the wear of the flanges of the leading wheels of locomotives tablets of grease, or of felt saturated with oil, have bean employed in Bavaria with success; applied so as to lubricate the flanges. It has been observed that the duration of the flanges is increased 50 per cent. by the lubrication.

That the link motion of Stephenson is not likely to be superceded for the valve gear of locomotives, has been proved by the results of a long series of experiments conducted by Professor Bauschinger, on a Bavarian Railway. Testing the comparative merits of the link motion and Meyer's valve-gear, the water consumed per HP. per hour was 37·4 lbs., with Meyer's gear, whilst with the link-motion it did not exceed 30½ lbs. The consumption of fuel were proportionally different. The comparative economy effected by the use of the link motion is ascribed—and, with much probability, justly ascribed—to the compression of exhaust steam, which takes place in cylinders fitted with that motion,

Continental railway engineers are tak ing active steps for preserving the boilers of locomotives. On the Thuringian lines of railway, the feed water, derived from the limestone and gypsum, rocks, has for some time been purified by treatment with chloride of barium and caustic lime, in settling tanks. An experimental trial of Herr A. Feldbacher's invention for preventing incrustation by lining the boiler with sheet copper, was made in a shunting engine at the Vienna terminus. The result of the trial showed that the system was completely successful in preserving the plates from incrustation and from corrosion.

Steam power has been substituted for the power of horses on light railways, with great economical advantage. At the iron works at Decazeville the cost has been reduced from 2·41*d.* per ton conveyed per mile by horses, to 1·56*d.* by locomotives. Similarly, in the coal mines at Doman, in Hungary, the cost has been reduced from 2*d.* per ton of coal drawn by horses, to a little over 1*d.* per ton drawn by locomotives.

The introduction of compound cylinders for locomotives, by M. Mallet, on the Bayonne and Biaritz Railway, is a new departure in the improvement of locomotives, which lend themselves readily to the compounding of the cylinders. It does not appear from the results of performance hitherto made public, that there is any distinctive economy of consumption in the compounded locomotives. The absence of economy is the natural consequence of the absence of steam-jackets or superheating appara tus. Meantime, M. Mallett has demonstrated the feasibility of the compound system in locomotives; economy will, no doubt, follow. In the same paper, M. Mallet summarizes the investigations of various experimentalists on the action and behavior of steam in the cylinders of locomotives.

According to an exhaustive report on the system of warming railway carriages in England and on the Continent, there is great diversity of opinion as to what constitutes the best system; though it is acknowledged that, in England, the common footwarmer is by general consent accepted as satisfactory, whilst it is the least expensive system. The reporter agrees in this conclusion.

The results of test-trials of turbine water-wheels on the continent and in America, appear to establish the superiority of the inward flow-kinds of turbine, originally designed by Professor James Thomson. Of these, the best recorded perfoemance is that of a Swain turbine, 72 inches in diameter, having a maximum gate of 13 inches, at Boot Cotton Mill, U. S. The turbine was tested by Mr. J. B. Francis. In the Swain turbine, the discharge is inward and downward. The receiving edges of the floats of the wheel are vertical, opposite the guide-blades, and the lower portions of the edges are bent into the form of a quadrant. Each float thus forms, with the surface of the adjoining float, an outlet which combines an inward and downward discharge. A maximum efficiency of 84 per cent. was attained with a 12-inch gate; with a 9-inch gate an efficiency of 83 per cent. was attained; at half-gate, 78 per cent.; and at quarter-gate, 61 per cent. These performances, taken tgether, are unrivalled by any other performance recorded in the abstracts. Girard turbines — tangential wheels specially suited for very high falls—are employed at the works of the St. Gothard tunnel. They have a diameter of 7 feet 10½ inches, and they make

160 turns per minute, with a maximum charge of water of 67 gallons per second. They are remarkable for the unprecedented head—279 feet—under which they are worked.

THE PROGRESS OF PUBLIC WORKS ENGINEERING.

THE first place must be assigned to the St. Louis Bridge over the Mississippi, designed by Mr. James Eads, M. Inst. C. E. This bridge, which carries a roadway above and a railway below, on girders formed of two arched steel tubes braced together, has two spans of 502 feet and a center span of 520 feet. It supplies a proof that, by the introduction of steel instead of iron, the weight of the superstructure of a bridge may be considerably diminished, its cost reduced, and large spans adopted even for carrying two roadways. The success of the undertaking furnishes a forcible argument in favor of removing the restrictions placed hitherto upon the employment of steel in this country. The method of erecting the bridge without scaffolding is worthy of notice. The girders were built out from the piers for a quarter of their length, and being fastened to the pier at the springing bore their own weight like a bracket, and the remaining portion was added by supporting their ends by iron ties from temporary erections on the piers. The principal object in forming such large spans was to diminish the number of piers, as the foundations were difficult and costly. The foundations for the piers of this bridge have been described in a paper by Mr. Francis Fox, M. Inst. C. E. At the time of their execution, the depth of the bottom of the east pier, 102 feet below water, was the greatest on record. Other instances of pneumatic foundations are given in the Abstracts, amongst which may be mentioned the résumé of various kinds of foundations for large bridges by Herr Funk, in which reference is made to the East River Bridge caisson, and to a bridge which is being built over the Lymfjord in North Jutland on iron caisson foundations reaching a depth of 118 feet below low water. The recent paper "On Foundations," by Professor Gaudard, renders further reference to these and other instances unnecessary.

The Kentucky river bridge is worthy of notice for the manner in which the center truss, 275 feet above low water and 375 feet span, was erected without scaffolding. Temporary piers were erected between the abutments and piers; the girders were then built out from the rock abutments on each side, being supported by the anchorage bolts till they reached the temporary piers. The building of the girders was next continued till the permanent piers and eventually the center of the centre span was reached, and the two portions united. The account of the Dwina bridge at Riga shows how large foundations have been put in under peculiar difficulties, exposed to a rapid stream and the breaking up of ice. The Moerdijk bridge over the Hollandsche Diep, whose foundations have been described in a paper by Sir J. G. N. Alleyne, Bart., M. Inst., C. E., is chiefly remarkable for its length, as the estuary where it crosses is 2,840 yards wide. The Marseilles swing bridge is an instance of a large and heavy bridge being moved round with perfect ease in three minutes by one man with the aid of hydraulic machinery.

Light railways and narrow-gauge railways have been introduced more extensively abroad than in this country, owing to the population being more scattered, to the absence of good roads, and to the importance of obtaining means of communication at the least possible cost. Also, in many mountainous districts steep gradients and sharp curves cannot be avoided in laying out railways, and the gauge and locomotives have to be made subservient to these requirements.

Amongst the railways where the standard gauge is adhered to the light railways in Hungary must be mentioned, as they represent the class of railway contemplated in the clause in the Regulation of Railways Act, 1868, authorizing the construction of light railways in this country, of which there are only three actually open for traffic, though others have been authorized, and one or more in course of construction. The two light railways in Hungary are each about twenty-nine miles long, and pass through a fertile district having bad roads. They are laid with rails weighing fifty-one pounds per yard, and cost a little over £4,000 per mile; the greater portion of the land was given for the railway; and the coun-

try being fairly level, the earthwork was small, the gradients are slight, and the curves are few, the sharpest having a radius of twenty chains. These afford a good instance of railways made economically, at the request and with the assistance of the landowners, for the purpose of obtaining the necessary means of communication. Lines like these are, in the author's opinion, the proper ones for supplying deficient railway accommodation in parts of England where the traffic would be unlikely to render lines constructed in the ordinary manner remunerative, and the hearty support of the landowners is an essential element in such undertakings. In Sweden this class of railway appears to have been extensively and successfully adopted, 1,730 miles being open for traffic or in course of construction.

The Uetli railway is a totally different type of line. It was made for the purpose of reaching the summit of the Uetli mountains, with steep inclines and sharp curves; the worst gradient is one in fourteen, and the sharpest curve 6.7 chains radius; the rails weigh sixty pounds per yard. It rises 1,310 feet in its length of 5.7 miles, and cost £11,100 per mile. It is worked by six-wheeled coupled engines, proving that inclines of one in fourteen can be safely ascended by locomotives. The train, composed of three carriages, performs the ascent in half an hour, and in its descent is kept under perfect control by air brakes.

The meter gauge has been adopted for several railways: for instance, on the line of Ergastiria in Greece, of Mokta-el-Hadid in Algeria, on three lines in Switzerland, and in France, Sardinia, and Austria. Bessemer steel rails were adopted in some of the above instances weighing from forty one to fifty-seven pounds per yard; the radius of the sharpest curve is about three chains, and the maximum gradient one in twenty-eight. In India this gauge has been adopted for all the narrow-gauge lines, of which there are 2,700 miles projected, and about one-third of these were completed in 1874. From a valuable collection of statistics about narrow-gauge railways, by M. Morandiere, it appears that the number of these lines has increased enormously of late years; the total number of miles opened for traffic in 1874 being 5,040, and 10,812 miles being under construction. The United States head the list with 2,040 miles open, and 7,552 in construction. India comes next, and then Canada, Australia, New Zealand, and Russia, whilst Great Britain is near the bottom of the list with twenty-six miles, being less than Java, which possesses thirty-four miles. There are twenty varieties of gauge on these lines, lying between one foot six inches and four feet; but next to the meter gauge, a gauge of three feet six inches appears the most common, having been adopted in Norway, Canada, the Cape, and on some American lines. It is natural that narrow-gauge lines have been little developed in England, France, Germany, and other continental countries, as the objections to the break of gauge in India, urged by several speakers in the discussion on Mr. Thornton's Paper on State Railways in India, apply with much greater force to the principal countries of Europe. An alteration in gauge can only be expedient in cases where it would be impossible to raise sufficient capital for constructing an ordinary-gauge line, and the choice lies between a narrow-gauge line and none at all. An instance of this occurred in the Grand Duchy of Oldenburg, where, in order to connect Ocholt and Westersede, each situated on railways of ordinary gauge, it was necessary, owing to the poverty of the country and the scarcity of the population, to construct the cheapest possible narrow-gauge line. This railway, which is nearly 4½ miles long, was constructed in 1876 in less than six months, at a cost, including rolling-stock, of £10,450; it was laid to a ¾-meter gauge, with Bessemer steel rails weighing twenty-five pounds per yard. Four trains run each way daily at the rate of about 13½ miles an hour; the fuel used is peat, and the daily expenses amount to only £1 9s. The Eibenthal mineral line furnishes an instance of a narow-gauge railway economically constructed through a hilly country. The gauge is two feet six inches; curves of one chain radius have been adopted, and when the line, at present worked by horses, is made available for locomotives, the total cost, including rolling-stock, will amount to only £1,857 per mile.

Riggenbach's rack system for sur-

mounting steep gradients, first adopted on the Rigi railway, has been extended to seven other railways. In the Ostermundingen railway the central rack-rail is only laid for the rising portion of the line. The locomotive works like an ordinary one along the level, and when it reaches the ascent of one in ten, the machinery is put in gear with the rack. A similar arrangement has been adopted on the Wasseralfingen railway, which has a gradient of one in 13, for about half a mile in length. Herr Wetli has invented a system for drawing trains up steep ininclines by the help of a drum, placed under the locomotive and connected with the driving-wheel, with spiral threads, running in opposite directions from the center to the sides of the drum, which wind along a pair of guide rails placed in a ∧ form at short distances apart. Herr Brockmann considers this system superior to that of the Fell railway, and also to that of the Rigi railway, in which the teeth of the central rack cannot be made strong enough to bear heavy traffic. Endless wire ropes are used for drawing the tram-cars up some steep streets in San Francisco, the connection between the rope and the car being so contrived that the rope is entirely below the road level, and causes no obstruction whatever to the ordinary street traffic.

Tramways along streets and public roads are being gradually extended. A concession was granted in 1873 for laying down sixty-six miles of tramways in Paris; and a steam tramway along the high road from Cassel to Wilhelmshöhe has been recently opened. Compressed air was tried first on this latter tramway, but proving a failure, Messrs. Merryweather's steam engines were substituted, which can draw two fully loaded cars up the gradient of 1 in 16.6. In Paris, Messrs. Merryweather's locomotive, the fireless or hot-water locomotive of M. Francq, and M. Mékarski's compressed-air engine, have been tried. It is stated that the use of the steam locomotive in Paris has not produced a saving of expense. This is contrary to the experience gained with similar engines at Copenhagen, where a saving of 40 per cent. was effected in the cost of working; with locomotives on the light Firmy railway, where a saving of one-third was made, and with Baldwin's steam car in America, where 18 per cent. was saved. The employment of fifteen fireless or thermo-specific engines on the New Orleans tramway effected an economy of 50 per cent. over the traction by horses. The performance of M. Francq's engine is to be further tested on the Paris tramways. M. Mékarski reckons that traction by his engine costs the same as with steam locomotives, and effects a saving of 25 per cent., or, according to his latest calculations, 40 per cent., on the total cost of traction with horses. A tramway in the surburb of Brussels is worked by a steam locomotive, entirely encased, to avoid frightening horses. It draws a weight of $2\frac{1}{2}$ tons up an incline of 1 in 50, at the rate of $7\frac{1}{2}$ miles an hour. Special compact locomotives, similarly disguised, have been used on tramways at Belfast and New Orleans; and locomotives have been introduced on some tramways in Italy.

A steam carriage, made entirely of steel, and weighing about four tons, for carrying twelve passengers, or drawing goods along an ordinary road, has been designed by M. Bollée. It accomplished a journey of 150 miles in eighteen hours. It can easily travel twelve miles an hour on the level, and $5\frac{1}{2}$ miles up an incline of 1 in 20, drawing a weight of four tons. It is easily steered through a town, and is said not to frighten horses, owing to the little noise it makes. M. Belpaire has designed a steamcarriage furnishing accommodation for fifty passengers. It can travel at the rate of forty-five miles per hour on the level, and twenty-five miles per hour up an incline of 1 in 62. The working expenses are reckoned at 1*d.* per mile run.

A description of the Heberlien continuous brake is given by M. Massauge. It was applied in 1874 to a train running between Brussels and Luxembourg, and acts on the principle of stopping a train by force accumulated during its motion. A new counter-pressure and vacuum brake, an improvement on the Le Chatelier system, is also described. These are the only continuous brakes to which reference is made in the Abstracts, but it is natural that less attention should have been paid to this subject abroad than in England, as—except on a few of the principal through routes—the speed of the

trains abroad is much less than in this country.

A new steam pile-driving machine, invented by Herr Lewicki, was used on the river Dwina regulation works, by which, on an average, fifty piles were driven per day. From a series of experiments it was found that the steam pile engine did twenty-eight times as much work as an ordinary pile engine worked by four men; and the cost is estimated at $8\frac{1}{4}d.$ per pile for the steam ram, and 4*s.* $6\frac{1}{2}d.$ for an ordinary ram. The peculiarity of the pile-driving apparatus described by Herr Weber is that a single steam engine controls several rams. The apparatus consists of an engine, of any kind with which driving bands can be used, the power distributor, and the ropes or chains for lifting the monkey. This invention was used on the Elbe at Dresden, for driving the piles of a quay wall, 541 yards long, where three pile-drivers were used. The work of driving about two thousand one hundred piles into a bed of rough gravel and stones, to a depth of from $3\frac{1}{2}$ to 7 feet, occupied thirty-two days. If ordinary hand pile-drivers had been used, the cost would have been increased by about 14 per cent., and the work would have lasted at least fifty-five days.

The powder-ram, invented by Mr. Shaw in the United States, and improved by Herr Riedinger, was used in constructing a bridge over the Elbe at Dresden. A cartridge of gunpowder thrown into a mortar, resting on the pile to be driven, is exploded by the fall of the ram, which drops into the mortar, and the gases formed in the explosion both drive the pile down and shoot up the ram to its original position. This ram is said to be rapid, efficient, and cheap. About twenty-seven piles could be driven per day, and the cost per pile, driven 7.2 feet, is reckoned at 8*s.* 6*d.* The great apparent difference in rapidity and cost between the results obtained with the powder-ram on the Elbe and the steam pile driver on the Dwina must be partially due to the difference in the bed of the two rivers —the bed of the Elbe consisting of rough gravel and stones, and a sort of quicksand overlying sand mixed with clay forming the Dwina river bed. Pile-driving in sand by the injection of water, from pipes carried down the sides of the piles, has proved very expeditious and efficient at the new harbor works at Calais, doing the work in about an eighth of the time occupied by the ordinary methods.

Though some important sewerage works, such as the drainage of Berlin, of Rome, and of Düsseldorf, are described in the Abstracts, there is no record of the introduction of any new system for dealing with sewage. The sewage of Berlin is to be disposed of by irrigation, and at Rome and Dusseldorf the sewage is discharged into the Tiber and the Rhine. At Paris considerable pollution of the Seine occurs from the influx of the sewage; the sluggish nature of the stream favors the deposit of the solid refuse matter, estimated at 130,000 tons annually, and dredging has to be resorted to for keeping open the mouths of the sewers. The Seine Pollution Commissioners unanimously rejected all chemical methods of purification, and expressed their conviction, from the results of irrigation tried on the plains of Gennevilliers, that irrigation was the proper method of disposing of the Paris sewage. These sandy plains have an area of 800 acres. The effluent water after irrigation contains only 1 part or 2 parts of nitrogen, having previously contained 44 parts. The improvement of the irrigated land in productiveness is very manifest. Vegetables, fruit-trees, and flowers grow well on the land, and the gross yield per acre is from £24 to £48 in the open fields, and in some well sheltered and cultivated parts it has reached £112. At Dantzic sewage irrigation experiments, commenced in 1871, have proved satisfactory both from a sanitary and agricultural point of view.

Hydraulic engineering occupies a prominent place in the Abstracts, and many important works, such as breakwaters, harbor improvements, reclamation works, reservoir dams, the regulation of rivers, the irrigation of dry or barren plains, and water-works for towns, are described; but most of the articles are valuable rather as records of successful applications, modifications, or extensions of well-known principles, than as embodying any special novelties of design. The dam across the valley of the Gileppe is a notable instance of a masonry dam made exceptionally strong for retaining a volume of water amounting, when the reser-

voir above it is full, to 16,010,000 cubic yards, and covering an area of 198 acres. It is 154 feet high, 216 feet broad at the base, and 49¼ feet broad and 771 feet long at the top. The Furens masonry dam, across the valley of Enfer, having a section which approximately coincides with the theoretical law as given in the article on the Gileppe dam, is 170 feet high, 110 feet broad at the base, and 9.8 feet broad at the top. These dams, however, whilst claiming notice on account of their size, are not quite without precedent, as the Alicante dam, nearly three centuries old, is 134½ feet high.

In supplying the Aix (provence) district with the water from the river Verdon, it was necessary to convey the water across the valley of St. Paul. As the construction of an aqueduct would have caused considerable delay, and involved a large expenditure, the novel expedient of carrying the water across the valley in an inverted siphon was adopted. Two wrought-iron pipes, 5 feet 9 inches in diameter, form the siphons; they are laid horizontally across the bottom of the valley for a length of 322 feet, and rise on each side of the valley, with inclinations of 1 in 2.44 and 1 in 2.7 for lengths of 251 and 276 feet respectively.

The drainage of Lake Fucino, rendered advisable from the absence of any natural outlet for the rain falling within its basin, is chiefly interesting for having been contemplated by Julius Cæsar, attempted by some of the Roman emperors, and again in the middle ages by Frederick II., and at length accomplished within the last ten years. The water is conveyed into the river Liris through an egg-shaped tunnel (19 feet by 13 feet) 6,887 yards long. In the construction of this tunnel twenty-eight shafts and two inclined galleries were made. The lands bordering on the lake, which were flooded in wet weather, and converted into unwholesome marshes in dry weather, have been greatly improved in value, and 35,000 acres have been reclaimed by lowering the waters of the lake.

The difficult task of keeping open a navigation channel at the mouth of the Mississippi has been successfully accomplished, by contracting the South Pass into a uniform channel, 850 feet wide, by wooden jetties on each side, the channel being for a time still further confined at the sea end by wing dams to increase the scour; and mattress sills have been sunk across the two other principal channels to divert more water into the South Pass. The navigation channel has already enlarged, and the original depth of 8 feet has been increased to 20 feet. It is anticipated that the scouring action will be still more effectual when the jetties are completed, and that, if a bar should tend to form at the sea end of the channel, an effectual remedy could be provided by the gradual extension of the jetties seaward at a moderate cost.

Though movable dams or weirs across rivers cannot be regarded as new inventions, having been for many years extensively employed on French rivers, and an instance being recorded of the erection of a movable dam in America in 1818, where they are now being gradually introduced, they are little known except in the simplest form in this country. M. Boule has designed a new form of dam, which he considers specially suitable for dams higher than 12 to 14 feet, the limits of the Poirée spar or needle system and of the turning floodgate of M. Chanoine. In M. Boule's design the shuttles are placed in tiers, between slight wrought-iron upright supports, 3½ feet apart, carrying the foot-path, so that the shuttles can be readily removed and replaced. In America shuttles hinged at the bottom, maintained in their places by props, and falling down flat on the apron of the weir when the props are removed, have been introduced. A somewhat similar principle has been adopted for some movable dams on the weirs Vizézi and Moingt; but in these dams the central pile supporting the shuttle is propped up, and the shuttles are composed of several boards laid horizontally, which can be removed successively, if the instantaneous removal of the whole dam, by releasing the prop supporting the central pile, is not necessary. A sluice-gate formed of narrow horizontal-jointed boards like a shutter, and winding up round a roller, has been put up at the weir of Notre-Dame-de-la-Garenne, on the river Seine. In the Mérienne dam on the Charente, by omitting the upper portion of the lower cheeks of the grooves in which the shuttles slide, the shuttles, when lifted to a certain height, being hinged to the racks, rise by the pressure

of the flood-waters towards a horizontal position, sinking again into their places when the flood has abated. A self-acting shutter for maintaining a constant head is described by Signor Nicorini.

French engineers have devoted considerable attention to the flow of rivers, and several valuable papers have been written on this subject, especially with reference to the Seine. The floods of the Po have also been carefully observed and recorded during the present century, furnishing important evidence of a gradual rise in the flood level. By a careful study of the Seine basin, and the flow of its tributaries, and by means of extensive daily rainfall observations, the late M. Belgrand has been able to predict at the beginning of the summer a drought in the autumn, and to forecast the rise of the Seine in flood time at Paris so as to give timely warning to the inhabitants along its banks. On the 14th March, 1876, M. Belgrand announced that the Seine, which was in flood, would reach a definite maximum height at Paris on the 17th of that month; and his prediction was fulfilled within half an inch. Floods of the tributaries of the Seine have been similarly predicted, and in the opinion of M. Belgrand the same system might be applied with success to many other rivers. The Author considers that a similar study of the flow of rivers in this country would furnish valuable information, at a time when the prevention of summer floods is becoming, in many districts, a matter of pressing importance.

The Ar-men lighthouse is being constructed on a reef of rocks near the Isle of Sein. The low level of the Ar-men rock on which the lighthouse rests, the rapid run of tide across the reef, and the exposure to the wind, rendered the foundations peculiarly difficult to put in; and the work was only accomplished by the energy of the native fisherman, who undertook the hazardous task of drilling the holes in the rock for receiving the iron dowels, to which the foundation courses of masonry were subsequently secured.

In this brief notice the Author has merely touched upon a few out of the many descriptions relating to public works abroad which have appeared in the Abstracts; but he trusts that those he has selected will suffice to show, that much may be learnt from a study of the practice and progress of public works engineering abroad, and in particular from the Americans in the matter of steel bridge construction, and from the French with reference to rivers.

Coal in China.—An attempt is contemplated to work coal mines in the neighborhood of Ching-men-Chow, not far from Ichang. Boring operations were commenced late last autumn. The coal-producing country appears to cover an extent of seventy-five square English miles, fifteen long by five broad. There are ten layers of coal, one above the other. The bed in Watzukow is estimated to be 500 English acres in extent; that at San-li-Kang to be one-fourth of its size. It is supposed that 1,200,000 tons of coal can be raised from Watzukow, and 800,000 from San-li-Kang, at the rate of 40,000 tons a year. The supply thus would last at least forty years. It is highly probable that further explorations will bring to light fresh beds, as these discoveries are the result of merely the first investigations. It should be mentioned that a few mines have been opened by the people living in the district, but they have not penetrated to the level of the best coal or largest seams. The bed at Wotzukow is 100 feet below the surface. The coal is just the same as the best American anthracite which is brought to China. Specimens of all the native and foreign coal procurable in China have been analyzed together and the new coal has shown itself superior to all for smelting purposes. The province of Hupei possesses several mines containing iron of excellent quality. If these are worked in connection with the coal mines large profits should be obtained, and if the example be followed in other provinces a source of wealth to the whole country will be opened up.

EFFECTIVE VENTILATION.

By H. C. STEVENS.

From "The Building News."

A HOUSE dwelling should afford us shelter from wind and rain, and while enabling us to obtain a comfortable temperature, should not deprive us of pure air for breathing. The ordinary house dwelling certainly provides us with shelter, but the requisite temperature and the pure air are obtained wastefully and with life-lowering insufficiency. The warming and ventilation of buildings is an intrinsically difficult subject, requiring unusual thoroughness and comprehensiveness in experiment. An illustration of the danger proceeding from the neglect of strict experimental investigation was afforded by the very general misconception with regard to the work performed by cowls in ventilation, which prevailed, until the president of this section (Captain Douglas Galton), with his colleagues upon the Ventilation Committee, published the results of their experiments at Kew. It is to be hoped that the very great public benefit conferred by that investigation will not be limited to the dispersion of the unfounded pretensions of the cowl supporters, but that it will inspire an attitude of sceptical inquiry towards ventilating appliances in general. Perhaps, also, a lack of precision in language has contributed to obstruct our progress. I may mention the word "draught" as an instance of this. In the popular sense of the word, a draught means a stream of air of highly uncomfortable and dangerous quality, and generally we find the effect of movement in the air and the impression derived from its quality are confounded one with the other! Air perfectly at rest—stagnant air—though its quality may be good, is not agreeable to us, and perhaps not healthy. Mere movement in good air is agreeable and stimulating; fanning is so. Thus, in ventilation, a current or draught is distinctly an object to be attained, but of course the draught must consist of pure air, agreeable in temperature and in its other qualities. I will define the ventilation of houses as the maintenance of the atmosphere of a dwelling in that condition of purity, temperature, movement, and moisture, which is found to be most agreeable to its inhabitants, and most conducive to their health and vigor. I think this definition is sound, although it goes beyond the limited sense in which the word is usually employed. The many modes of ventilating at present practised may be classified as belonging to three fairly distinct principles. 1st. We have ventilation by the natural or spontaneous method, or, as I prefer to call it, ventilation by the *exterior wind agency*, 2nd. We have ventilation by the operation of gravity obtained in ventilation by *heat agency*. 3rd. We have ventilation by mechanical appliances, as blowers, fans, or pumps, which may be described as *mechanical agency*.

Ventilation, dependent upon the *Exterior Wind agency* principle, is the form commonly employed for the introduction of fresh air into house dwellings in this country. The appliances for it are various in character. Among them are very numerous contrivances applied to holes in walls of buildings. To this principle also belongs the introduction of fresh air by tubes which convey it into the interior of dwellings in a manner to cause the least annoyance and discomfort. Tubes for extracting air open at the top, or surmounted by a cowl, whether used in conjunction with some mode for admitting exterior air or not, must also be placed in this class. In many of these contrivances the action claimed for them as proceeding by their operation, from difference in temperature between interior and exterior air, does take effect to some extent, but such action is altogether inconsiderable when compared with the influence exerted by the exterior wind currents to which the tubes are exposed. I would ask your earnest consideration as to the value and expediency of relying at all upon the force of natural wind currents for a supply of air for breathing purposes, and it would be a great step in advance to acquire clear views upon the merit of this form of ventilation. A leading cowl manufacturer in his prospectus says that the wind has an average velocity of ten

miles an hour in this country—it is easy to calculate an average, but to be of use for ventilation that average speed should have a reasonably enduring continuance, it should represent a normal condition of the wind force—the wind in this country actually varies from about 1 to 30 miles per hour, however seldom such extremes may be touched. I live in the midst of trees and note the play of their foliage. I have ventilators based upon this exterior wind agency principle in some of my rooms, and I have carefully watched their action; both the foliage of the trees and the action of the ventilators attest the incessant and wide variation of the force exerted on them; further than this these appliances only deliver air when the wind is blowing on their side of the house, then those on the windward side deliver air rapidly, while from those on the leeward side no current is perceptible. It is true that when a fire is burning air will enter through them to compensate for that driven out by the fire currents; but the quantity and force will depend mainly upon the exterior wind currents. At one time air enters sufficient to replace that driven out by the fire currents; at another, irrespective of the action of the fire, the wind rushes in in an unwelcome torrent. In summer when there is in the temperature of the interior and the exterior of our dwellings but little difference, ventilation by appliances, really dependent upon "exterior wind agency," are often quite inoperative. The air is then frequently so still that a candle flame will suffer hardly any apparent deflection, even when passed through an open window, and at such times no current can be detected from tubes or other openings communicating with the external air. It is, however, precisely at such times that the need for the introduction of fresh air at a reliable rate is most felt. On the other hand, when the wind is driven through every chink and cranny of our houses, the air-tubes on this principle deliver a superfluous and unbearable addition to it. For the ventilation of sewers, cellars, and for what I can best describe as *air-cleansing work*, the fitful, but from time to time powerful and thorough sweeping obtained from the full force of an exterior wind current, is most valuable; but our respiratory process is regular and uniform, and some thing like a corresponding uniformity in the quantity and quality of the air supplied to our dwellings is required by us. If this be conceded, I submit that the admission of the force of wind currents is pernicious, and must frustrate the attainment of reliable and uniform ventilation.

Ventilation on the second principle, by *Heat Agency*, is a great improvement on that obtained by exterior wind agency; but if applied to extracting air by chimney draughts, the admission of air is usually allowed to depend upon appliances largely controlled by external wind currents. In summer the plan of propelling air by heat ventilation cannot well be carried out, and the stoves devised for it usually operate at that season of the year upon the external wind agency principle with its inaction during the hot calms of summer, and excessive action when the natural leakage of our houses introduces air enough.

It is by the employment of the third principle, that of *Mechanical Agency*, that we can alone become masters of the situation—able to introduce air at any required rate, and, at the same time, do very much towards raising its life-sustaining value. By the use of fans, blowers or pumps, the quantity of air admitted is placed under easy and immediate control, and its condition can be modified so as to bring it to a near approximation with any desired standard. To furnish power for blowing or pumping air into large buildings is simple enough; but its application to the needs of private dwellings in a manner sufficiently simple, automatic, and inexpensive, is surrounded with considerable difficulty. The ordinary means for the application of power appeared incapable of furnishing a flow of energy in a form sufficiently attenuated for a machine required to do work as undeviating, and as constant as the process of respiration within ourselves. The descent of heavy weights, governed by clock work movement, affords an arrangement by which a considerable amount of energy can be stored and liberated with uniformity at any desired rate.

A descending weight of one ton might run in a shaft by the side of a house from the top to the bottom, or from the basement downwards in a tube to any con-

venient depth, or by combining the two, a vertical fall of many feet would be obtained. The weight could be raised by multiplying pullies by hand power, or by any form of engine, steam, wind, hot-air or gas; but by using gas or hot-air engines, it would probably not be difficult to contrive some continuous automatic arrangement for starting the engine to wind up the weight. I have made trials in ventilation by using the fall of a weight (2 cwt.) descending 30ft. in connection with a small fan blower for the ventilation of a room, and I have also tried by means of the same machine the ventilation of a number of inclosed spaces representing in number aud arrangement the rooms of a house. Another mode would be that of employing two cylinders of capacity sufficient to give the required force, running over pulleys, made to fill automatically with water at the highest cistern of the house, and to empty themselves into a lower cistern. The two cylinders would rise and fall alternately, so as to offer a continuous exertion of power. My results so far have been encouraging, but they are not sufficiently matured to lay before you. There is a plan patented by Messrs. Verity and Co., by which power is obtained from a very small stream of water from a cistern at the top of a dwelling-house, or direct from the water company's mains. Wherever water is available, and the amount of ventilation required is small, this plan is exceedingly convenient and inexpensive. Hot air-engines and gas-engines are now manufactured for machinists of a power low enough to enable them to be employed to give direct motion to a fan or blower without the aid of any intermediary. By using hot-air engines heated by gas a considerable, down to a very small, exertion of power can be obtained, which, with some precautions, may be expected to act with the permanent and regular motion absolutely necessary in any arrangement for ventilation.

After giving a good deal of consideration to the subject, and working at it in various ways by experiment, I came to the conclusion that we should not shrink from endeavoring to grasp, for an application to domestic buildings, the teaching afforded by the results of the best examples of ventilation in this country, such as the Houses of Parliament for instance. In short, that opportunities presented by a thorough and completely controllable system of air circulation should be utilized to the utmost, aiming at more than a mere replacement of breathed by unbreathed air. Pure air united to the climatic conditions most agreeable and desirable for us is what is wanted. One process should enable us in winter time to secure a summer-like condition of air, as well as to maintain a high standard of purity in it throughout the whole of our dwellings.

Nothing but long habit could reconcile us to the extraordinary barbarism of our present arrangements, a small patch of heat and a large space of cold in each room, by which the arrangement of the dinner-table and how to sit at work in one's study become problems it is impossible to solve; but such riddles are mere trifles and quite harmless compared with the important household question as to what condition or period of life, or what infirmity in health, constitutes a valid claim for the indulgence of a fire in the bed-room. The air of half our rooms is that of a cross between the atmosphere of a marsh and a glacier, and many of us leave a warm drawing-room to undress and sleep and dress in the morning during winter under circumstances of pain and peril. Our houses surely cost us money enough to build or to rent; why should we not keep the whole of them in a habitable condition? The labor, much of it of a very heavy and disagreeable kind, necessary for the average number of fires required in a large house is probably equal in the aggregate to the time and care which would suffice to produce, with the proper appliances, any desired climatic condition throughout an entire house; and though the amount of attention required to carry out warming and ventilation in the form I contemplate may be too considerable for application separately to small houses, why could not such houses, if built in rows, or very near together, be supplied with pure air of summer climate from one source, taking precautions to prevent the loss of heat by the employment of non-conducting coats to the main pipes. The important item of saving effected by the avoidance of the destruction caused by stoves and open fires to the furniture, carpets, and hangings, must not be left

out of consideration. In point of original cost it is probable that the numerous stoves, with all their attendant paraphernalia of mantelpiece, fenders, hearths, &c., in a house of twenty rooms, would cost as much in the first instance as the machinery for heating and ventilating, while in planning new buildings the arrangements for such a form of ventilation would not add materially to their cost.

I intend to apply these views to some buildings of different requirements I am about to erect. My endeavor will be firstly directed towards means by which the air used for circulation shall be endowed with qualities which will make those buildings comfortable and healthy at all seasons, and in all temperatures. The air will enter freely into a large chamber in which the whole of the appliances for heating the air, moistening it to obtain an agreeable dew point, filtering it from dirt and blacks, and finally dispatching it, by means of a blower or other mode, at any desired speed, will be carried on. The details of arrangement for the circulation of air are, in some respects, those already well known, but in others they are of a special character. Great precaution is necessary to avoid any risk of injury to the quality or agreeableness of the air from the mode of heating it; but a chief cause of the difference experienced in the quality of air heated in various ways will commonly be found to be attributable to a neglect of its dew-point.

In conclusion, I submit that in experiment in ventilation we should keep in view, as desirable of attainment, the following:—

1. That the force of the current, the rate of supply, and quantity of air supplied for the purposes of ventilation of dwellings must be entirely under control.

2. That the quality of all the air supplied for ventilation of dwellings should be capable of easy approximation to any condition of temperature and moisture deemed desirable at any season of the year.

3. That greater influence should be exerted upon the circulation of air in dwellings by the apparatus for inflow than by the means for out-flow of air.

It is stated that in consequence of the new German customs duties legislation, and of other circumstances, a project for the improvement of the Russian railways and the Baltic ports will be speedily carried out. St. Petersburg is to be made a seaport by means of a maritime canal, which will permit the large vessels obliged now to stop at Cronstadt to take in and discharge their cargoes in the Russian capital. The works necessary to make St. Petersburg the largest seaport in the Baltic will be executed in eight years at a cost of 8,000,000 roubles. The port of Libau is also to be enlarged and deepened.

A large amount of money is being expended by the Queensland Government on the Fitzroy River, Rockhampton, in order to make it navigable for coasting steamers and other vessels at all states of the tide. From a report just made by Mr. Nesbit, the Chief Engineer of the Harbors and Rivers Department—it appears that exclusive of the first cost of dredging plant, there has already been expended £35,680, and it is estimated that a further sum £67,467 will be required to complete the work to Central Island, and that if the further works recommended by Mr. Nesbit are carried out, the total expenditure on the improvement of the navigation of this river will amount to more than £145,000.

The Swedish State Railways last summer adopted for the carriage of meat, game, fruit, milk, and similar commodities a number of so-called cooling wagons of a novel type. On the exterior they are painted a pure and brilliant white. In the interior they are fitted with two large ice holders, which are filled from holes in the roof of the wagon. As the ice melts the iced water is conducted through the body of the wagon by iron tubes, which keep the interior at a constantly low degree of temperature. The size of the floor of each wagon is eleven square metres, and the weight they can cach carry is eight tons.

ON CAST IRON FOR ENGINEERING PURPOSES.

By Mr. J. S. Brodie (read before the Liverpool Engineering Society).

From "Iron."

Cast iron is now in such general use, it enters so largely into the designs of almost every practical engineer, either civil or mechanical, in the construction of roofs, bridges, machinery, &c., and innumerable other purposes, that no apology need be made for introducing a few practical considerations under the above heading to the attention of this society. Before going into the nature and properties of the material itself, a very brief historical sketch of its development may not be out of place. Without investigating the somewhat debatable question of the prehistoric use of iron, a subject of inquiry more fitting for imaginative archæologists than for matter of fact engineers, it may be taken as being capable of very certain proof that the use of cast iron for structural purposes was almost, if not altogether, unknown before the time of Smeaton. That celebrated engineer is quoted by Tredgold, in his book on "The Strength of Cast Iron," as having in the year 1755 made use of cast iron for structural purposes in connection with mill work in the North of England, but for what particular purpose does not appear. It would also seem to have been used to some extent by Savery and Newcomen in their steam engine and pumps, the cylinders of Newcomen's steam engine being made of cast iron. The art of founding would no doubt be very crude and imperfect in those early attempts, and many improvements were made in this respect by Smeaton, Wilkinson, Tredgold, &c. The first notable example of the application of cast iron on a large scale to bridge purposes was the Coal-brook-dale Viaduct, erected about the year 1755, which crosses the River Severn between Madeley and Brosely. The bridge was erected from castings made by a Mr. Wilkinson, a local ironmaster, from designs prepared by a Mr. Pritchard, an architect of Shrewsbury. Forty-two years afterwards the famous cast iron bridge over the River Wear, at Sunderland, was erected from designs prepared by Thomas Payne. According to the late Sir William Fairbairn, the first application of cast iron beams for the purpose of house construction was that of a fireproof cotton mill erected in the year 1801 by Messrs. Phillips and Lee, of Manchester. The iron beams and columns were designed by Messrs. Boulton and Watt. No improvement seems to have been made on this erection, which continued as a model for all similar structures until Mr. Eaton Hodgkinson commenced his well-known series of experiments into the strength of iron beams and columns at Fairbairn's Works at Manchester in 1827. The results of those experiments were sufficient to considerably modify all previously received opinions on the strength and other properties of cast iron, more especially the tensile strength, which was found by Hodgkinson to have been overstated on the authority of Tredgold, by more than double the actual breaking strength. It is evident, therefore, that the beams and columns designed by Messrs. Boulton and Watt on Tredgold's data must have been allowed a very liberal factor of safety. The adoption of cast iron into general use as a building material for bridges, mill work, &c., may be said to have commenced on the experimental foundation so successfully laid by Mr. Eaton Hodgkinson, who is even up to the present time the chief authority on the strength of cast iron beams and columns.

With this brief introductory historical sketch, the more immediate subject of this paper, viz., "The Nature, Strength, and other Properties of the Material," may now be proceeded with:—Cast iron is obtained by smelting iron ore together with coal, or coke, and limestone, in a blast furnace, at a temperature sufficient to melt the most refractory of those substances. The contents of the furnace are reduced by heat to cast iron and slag, the latter being a glassy substance formed by a combination of the limestone, the earthy ingredients of the ore, and the impurities in the fuel, and is

practically a waste product. The cast iron run direct from the blast furnace is usually known as "pig" iron. This pig iron is found on analysis to consist of from 90 to 95 per cent. of iron; from 8 to 2 per cent., by weight, of silicon, sulphur, and other impurities, and from 5 to 2 per cent., by weight, of carbon, either in a state of chemical combination with the iron, producing *white* cast iron; or the carbon may be partly combined and partly mixed in the state of graphite, when it is known as *grey* cast iron.

White cast iron is, as its name implies, of a silvery white, close grained, hard, and very brittle. It is produced by a comparatively smaller quantity of fuel being used in the blast furnace and a corresponding low temperature. It is easily discerned on being tapped from the furnace by its sluggish appearance, and by the way in which it scintillates into the air while running into the moulds. It is subdivided into *granular* and *crystalline*, according to its texture. *Granular* cast iron is sometimes used for castings by being converted into grey cast iron by remelting and slow cooling. *Crystalline* iron, being used only for forge purposes, will not be further noticed. *Grey* cast iron is subdivided into Nos. 1, 2, 3, and sometimes 4. No. 1 is the product of a high temperature in the furnace, caused by a large amount of fuel in the charge. It is the weakest, and is of a large texture, while Nos. 2, 3, and 4, produced at lower temperatures, are successively harder, and of a much closer texture. Grey often merges into white cast iron very gradually, so that the two varieties may often be found in the same fracture. In this case it is known as *mottled* iron, and this class of iron is often found to be very strong.

Generally, it may be stated that white cast iron is the product of a comparatively low temperature caused by deficiency of fuel, while grey is produced at a high temperature and with a larger supply of fuel, and all the intermediate classes from bright white to dark-blue grey are produced by modifications of those conditions. The author's own experience has been that with a large close-topped furnace in average working order and a high temperature of blast, an allowance of about 19 cwt. of coke for white iron, and 21 cwt. for grey, per ton of iron produced, is sufficient.

Without going too minutely into the different qualifications of the various brands for special engineering work, or insisting too much on special mixtures for producing different castings, a short review of the different kinds and their general applicability will now be given. No. 1 is remarkable for the smoothness of its surface in the pig, and melts at a *lower* temperature than any of the others. It contains the largest amount of uncombined carbon, and, owing to its greater liquidity, produces the sharpest and best defined castings where strength is not required, as, for example, in ornamental work. No. 1, when broken, shows a blue-grey color with a coarse grain. No. 2 is less smooth on its surface than No. 1, and is not so liquid when melted. It is closer grained, lighter in the shade when fractured, and is more tenacious. It can be easily worked in the machine, and is preferred for ornamental castings of more strength, such as garden rails, gratings, &c. Nos. 3 and 4 are the kinds most frequently used, either separately or together, or mixed with scrap, for engineering purposes. No. 3 is close grained, the fracture showing a little mottled on the outside, and is distinctly hard and strong while retaining in a great measure the toughness of softer iron. It is invariably selected for heavy shafts, wheels and cylinders, when mixed with soft scrap, and other cases where variable strains are to be expected. No. 4, on the other hand, being much stronger and harder, though less tenacions, is best adapted for very heavy massive castings where the load is more uniform, such as bed-plates, columns, girders, &c., and should never be used where much machine work is intended to be put upon it, as it is too hard for this purpose. *Mottled* iron is seldom used by itself for engineering castings on account of its great hardness, but it is often used with great advantage in mixing it with softer iron to obtain strength with tenacity.

In the early use of cast iron, engineers frequently specified the mixture from which their castings should be made. This was soon found to be inadvisable, for, on the one hand, the founder having deposited the proportions of the mixture specified in the cupola, very properly

considered his responsibility to be then in a great measure at an end, throwing all the blame upon the mixture for any damage that might happen to the castings in the various stages of the foundry. On the other hand, the same number of iron was found to differ so much in quality in the same works (to say nothing of the difference between separate works) that it was found in every instance to be very unsatisfactory. Engineers were therefore led to adopt in their specifications standards of strength, appearance, sound, &c., to the finished casting, leaving the founder to employ what pigs he might see fit to produce iron, fulfilling the tests specified when cast.

In considering the subject of the strength of cast iron, only a very general outline can be attempted within the limits of a paper like the present, and the author must refer to the published results of the various experimenters for further detailed information, merely giving general conclusions that have been arrived at from time to time. The subject of strength falls naturally under four separate heads, viz.: Tension, Compression, Transverse Loading, and Torsion. First, as to the tensile strength or resistance to tearing asunder of cast iron. As has already been stated, Tredgold was amongst the first to make experiments on the tensile strength of cast iron, some time about the year 1821. He seems to have made a few owing to the contradictory nature of the tests by direct pulling asunder, but results so obtained, he abandoned them and proceeded to deduce the tensile strength from transverse breaking experiments. For this purpose he caused bars to be supported horizontally at the two ends and loaded in the center, and, assuming the elasticity to be perfect, he deduced from the weight placed on the centre of the bar the tensile stress at the lowest point, thereby obtaining a breaking tensile stress of from 18 to 22 tons per square inch. The fallacy of the results thus obtained were afterwards pointed out by Mr. Eaton Hodgkinson, who showed that owing to the imperfect elasticity of the iron, the neutral line, instead of remaining in the centre of the section, came very near the top at the time of fracture, thereby upsetting Tredgold's calculations. Mr. Hodgkinson then proceeded to make a number of direct experiments on the tensile strength of cast iron bars of from 1 to 4 square inches sectional area, with the following results:

Maximum breaking weight per square inch	9.75 tons
Minimum breaking weight per square inch	6.00 "
Mean of all the experiments	7.37 "

Thus it will be seen that if the iron experimented upon by Mr. Hodgkinson was at all of similar quality to that made use of by Tredgold, of which there seems to be every probability that it was at least equal, then the results of Tredgold were nearly three times in excess of the actual tensile strength. In 1849 the Royal Commission appointed to inquire into the application of iron for railway structures, instructed Mr. Hodgkinson to make further experiments on the strength of cast iron. In connection with this inquiry he tested eighty-one specimens of seventeen different kinds of cast iron, of from three to four inches of sectional area, by direct tearing asunder, with the following results, viz.:

Maximum breaking weight per square inch	10.5 tons
Minimum breaking weight per square inch	4.9 "
Mean of all the experiments	6.8 "

In 1856 the Government authorities made a number of experiments on different kinds of cast iron at the Royal Gun Factory, Woolwich Arsenal. The results were published in a Blue Book in 1858. No attempt was made in these experiments to test mixtures, and it must be observed that as specimens of pig-iron were openly invited from different makers for the purpose of testing, it is not surprising to find the results higher than Mr. Hodgkinson, whose specimens were taken indiscriminately. Eight hundred and fifty specimens, 1.3 inches in diameter, cast in one melting from the pig iron thus obtained, were tested by direct tearing asunder, with results as follows:

Maximum breaking weight per square inch	15.3 tons
Minimum breaking weight per square inch	4.2 "
Mean breaking weight per square	10.4 "

Professor Rankine, taking the average of a large number of experiments from different sources, gives:

Maximum breaking weight per square inch	13	tons
Minimum breaking weight per square	6	"
Mean breaking weight per square inch	7.36	"

And this will found to be the strength usually adopted by English engineers for ordinary purposes; higher tests being of course required for special work, and when special measures are taken in order to produce a greater tenacity.

Resistance to Crushing.—At an early period cast iron was found to have much greater resistance to crushing than to tearing asunder. As early as 1818 the results of experiments were communicated to the Royal Society by Mr. Rennie on cast iron to resist crushing, which showed a resistance of from 33 to 90 tons per square inch to produce fracture. Mr. Hodgkinson took this branch of his researches up with his accustomed thoroughness, first making a series of thirteen experiments with cylinders $\frac{1}{4}$ inch to $\frac{3}{4}$ inch in diameter, and also with rectangular prisms, with a height of from $1\frac{1}{2}$ to three times the diameter. The experiments were made from different descriptions of cast iron, with results as follows:

Maximum crushing weight per square inch	64.92	tons
Minimum crushing weight per square inch	38 5	"
Mean crushing weight per square inch	48.0	"

Mr. Hodgkinson made experiments on crushing from the same casts as his tensile experiments above quoted from the report of the Iron Structures Commission of 1849, as follows:

Maximum crushing weight per square inch	53.8	tons
Minimum crushing weight per square inch	24.7	"
Mean crushing weight per square inch	38.5	"

The Woolwich authorities made experiments in crushing simultaneously with their tensile experiments, and under the same conditions. Two hundred and seventy-three specimens were treated in a cylindrical form, 0.6 inch diameter, and 1.3 inch high, as follows:

Maximum crushing weight per square inch	62.5	tons
Minimum crushing weight per square inch	19.8	"
Mean crushing weight per square inch	40.6	"

And, finally, Professor Rankine, collating from various authorities, gives

Maximum crushing weight per square inch	64.7	tons
Minimum crushing weight per square inch	36.6	"
Mean crushing weight per square inch	50.0	"

Transverse Strength.—A considerable number of experiments were made at an early date on the transverse strength of cast iron. Those early investigations were made with a view to the application of the material in the form of beams to carry transverse loads. Tredgold quotes several experiments as made previous to his own which have been already alluded to. But the subject was still in considerable uncertainty previous to the searching and valuable experiments of Messrs. Fairbairn & Hodgkinson on the strength of cast iron beams in 1827. These experiments were made chiefly with a view of determining the best and most economical form of section and outline for joist beams for warehouses and mills. But as this subject is a wide field of inquiry of itself, and is without the limits of the purposes of this paper, the author will not enter further into it than to consider the case of a rectangular bar, such as is most frequently used for testing purposes. The author's own practice is to specify a bar of 1 inch square sectional area to be placed on supports 3 feet apart, and is weighted in the center of the bar with the amount specified, which depends upon the quality of the material required. Other dimensions are frequently used by engineers, but the results are easily comparable by the use of the well-known formulæ for rectangular beams, supported at the ends and loaded at the center when

b = breadth
d = depth
c = length between supports
} All in inches.

W = breaking weight in tons applied at center.

S = constant denoting the strength of the materials in tons, then $S = \frac{Wc}{bd^2}$.

In Messrs. Fairbairn's & Hodgkinson's

experiments on the transverse strength of cast iron about 270 bars were broken, each bar being 1 inch square and 5 feet in length, placed on supports 4 feet 6 inches apart, and the following results reduced to the value S, in the above formulæ, were obtained, viz:

Maximum transverse breaking weight.	14	tons
Minimum " " .	8.6	"
Mean " " .	10.9	"

The iron for these experiments was taken from nearly sixty different works in the United Kingdom. Experiments on transverse breaking were also made at Woolwich with bars about 2 inches square and 20 inches between the points of support, on 564 specimens, the results being again reduced to the value S, thus:

Maximum transverse breaking weight.	20	tons
Minimum " " .	4.6	"
Mean " " .	12.6	"

Transverse experiments were made by Mr. Robert Stephenson in 1846-47, to determine the most suitable mixtures to be used for casting the large arched girders of the high-level bridge at Newcastle-on-Tyne. The test bars used were 2 inches square in section, placed on supports 3 feet apart, with the following results, viz:

Maximum transverse breaking weight.	17.2	tons
Minimum " " .	11.0	"
Mean " (not given) .		

A series of tests recently made by the author for cast iron work, made under his supervision, gave as the result of fourteen test bars, representing the same number of meltings, 1 inch square, placed on supports 3 feet apart, and loaded on the center.

Maximum	13.51	tons
Minimum	12.14	"
Mean	12.61	"

of which specimens were exhibited. In testing castings by means of cast specimens it must, of course, always be borne in mind that, owing to the crystallization of chilling of the surface of the iron, a small casting must always be stronger than a larger one, since in the former case the crystallized skin bears to the whole sectional area a much greater proportion than in the latter. Experiments made on this part of the subject have shown that a bar 3 inches square is only relatively half as strong as a bar 1 inch square. In order, therefore, to obtain reliable results from the test bars, the engineer should always specify for a sectional area of the bar equal to the thickest parts of the casting subject to transverse strain.

Torsional strength.—Very few experiments have been made on the torsional strength of cast iron. Those who have experimented have usually done so upon cylindrical specimens fixed at one end and twisted at the other, and the author, for the sake of simplicity of comparison, will confine himself to round bars. In this case let

d=diameter of bar in inches
M=moment of twisting force which will break the bar (in inch bars)
c=co-fficient of resistance to wrenching, in tons

then $C \propto \frac{M}{d^3}$ for the same material.

A few experiments were made by Tredgold on cast-iron shafts, varying from 4 to 4½ inches in diameter, which give values for C, the resistance to wrenching, as follows:

Maximum=2.56 tons
Minimum=2.00 "
Mean =2.34 "

It also appears, from experiments that Mr. Rennie made on round bars, that

Maximum=3.68 tons
Minimum=2.13 "
Mean =2.85 "

From the Blue Book containing the Woolwich experiments it appears that tests were made for torsional strength upon cylinders 3 inches long in the twisted part and 1.8 inches in diameter. 276 specimens were tried as follows:

Maximum=4.4 tons
Minimum =1.65 "
Mean =2.70 "

Having now considered the ultimate strength of cast iron under the several conditions in which it is usually applied for engineering structures, it remains to be seen how these data can be applied to practical use. In order to arrive at reliable conclusions for this, it will first be necessary to see what is the behavior of cast iron under varying weights, less than the ultimate breaking weights; behavior under vibration; and also under varying temperatures within probable working limits. The measure of the elasticity of a material, such as cast iron,

may be defined as being the proportion which the temporary extension or compression in the length of any bar, during the application of a force of known amount tending to stretch or compress the bar, bears to its original length, when the elasticity remains perfect, or the bar returns to its original length as soon as the force is removed; while the measure of ductility is the proportion which the permanent extension of any bar after being torn asunder bears to its original length. In the former case the measure is called the modulus of elasticity, which is the weight, usually in pounds, which will stretch a bar 1 inch square to twice its original length, supposing the elasticity to remain perfect; and it will thus be seen to be entirely an imaginary quantity, since it is almost unnecessary to observe that scarcely any material has elastic limits approaching this ideal. Notwithstanding that a considerable number of experiments have been made on the elasticity of cast iron, the subject yet remains in considerable obscurity; for while Tredgold inferred from his experiments that its elasticity was not impaired by being loaded to one-third of its breaking weight, Mr. Hodgkinson showed that the limit of elasticity was very much below this, but that although a "set" would be produced at this lower limit, yet that the material was not permanently injured by being loaded up to that limit. Acting on this data the Board of Trade Regulations permit cast iron to be loaded with a dead load of one-third of the ultimate breaking weight. Professor Rankine, collating from various sources, gives a modulus of 17 millions, corresponding to a tensile strength of 7.34 tons. The author, from his own experiments on cast-iron bars loaded transversely, has deduced moduli varying from $15\frac{1}{2}$ to 18 millions, or an average of $16\frac{3}{4}$ millions of 14 test bars. The figures given by Rankine may therefore be taken as tolerably near, although it is much to be regretted that more reliable data are not available on this part of the subject, which seems to have been very generally neglected by investigators, and the ultimate deflection taken instead, which is really no measure whatever of the elasticity of cast iron, but is simply some indication of ductility which will now be considered.

The *ductility* of cast iron is, as must be apparent from the nature of the material, very small; the most careful experiments showing $\frac{1}{8}$ to $\frac{1}{8}$ per cent., or an average of $\frac{1}{6}$ per cent. The author, from an average of fourteen experiments on different meltings, found it about $\frac{1}{4}$ per cent. on the specimens exhibited. The smallness of this will be more clearly impressed upon the mind when we remember that wrought iron and mild steel have from 8 to 25 per cent. of ductility. Still, small as it may appear, it should always be carefully noted in testing, as it has a most important bearing on the durability of the casting.

Then as to *vibrations:*—It has been found by several independent investigators that cast iron beams have not been appreciably impaired in strength by the continuance for many years of strains upon them when moderately loaded to within the elastic limits.

With regard to variations in temperature, it appears from the report of the Iron Structures Commission, already alluded to, that there is no appreciable difference in the strength of cast iron from freezing point to about 600° Fah.

A few remarks on the means adopted to protect the surface of castings may not be out of place here. Although cast iron suffers much less from oxidation than wrought iron or steel, owing to the greater proportion of pure iron in the latter, still it must have been in the experience of every one familiar with its use how very soon, comparatively, it is eaten away under certain circumstances. Quite recently the author was called upon to examine some cast iron spouting at one of the Corporation yards in Mill Lane, when he found it completely eaten through, although it had not been in use for more than ten years. Large holes were found in the bottom of the eaves spouting, which in some places was not more substantial than brown paper, while the down-spouts were so much oxidized as to fall to pieces on being lightly touched. Since then the author has been informed of instances in the City of Glasgow in which cast-iron spouting has been rendered useless through oxidation in less than three years. For castings of an ornamental description, among the most successful of the protective coverings is the electrotyping process with metals which have a

less affinity for oxygen than for iron—such as copper, zinc, nickel, and sometimes silver and gold. The first-named, copper, has been most extensively employed, as it gives to the largest castings the appearance of bronze—many so-called bronze castings being only cast iron with a copper coating. *Nickel*, on the other hand, by giving to the casting an agreeable white grey color, and having a very small affinity for oxygen, is more used for large exposed surfaces, since it does not oxidize in contact with the air, or even moisture. The *tin* and *zinc* coatings, or "galvanizing," produced by deposition from chloride solutions, are among the most extensively adopted of the metallic coverings. Lastly, of the numerous patented and otherwise infallible paints and varnishes, the new varnish, "Diamond Color," is claimed by its promoters to give a good protection against rust, and certainly it possesses one great merit on account of its greyish blue tint, which gives to the coating its natural appearance, making iron look like iron, which must always be a source of satisfaction. Of the various paints it is only necessary to mention the coal-tar, linseed oil, and common oil paints, all of which are very useful and serviceable, but must be periodically renewed.

The examples of cast iron erections which the author now submits are to be taken more as examples representing the progress of our knowledge of cast iron than as being in any sense a complete list. The first iron bridge erected in this country was, as previously stated, that on the Severn, at Coal-brook-dale. The span is 100 feet, and is semi-circular. The great ribs of the arch, each cast in two pieces, meeting at the crown, are about 75 feet in length. The second iron bridge was built on the same river at Buildwas, in 1795, under the supervision of that father of modern engineering—Thomas Telford. This bridge has a span of 130 feet, and has two cast-iron side arches with 34 feet rise, from which the platform of the bridge is suspended as in a suspension bridge. The third iron bridge was the Sunderland bridge, designed by the celebrated writer, Thomas Payne. The span of this bridge was 236 feet; rise, 34 feet; width of roadway, 22 feet; and there were six ribs. In this bridge the principle of construction adopted differs entirely from the other two just mentioned. Instead of going upon the arch-girder system the iron was treated precisely as if it had been stone, and therefore each rib instead of being in two parts was made in 125 parts. In the place of stone voussoirs forming the arch of a stone bridge, what we may call cast-iron framed voussoirs, each about 2 feet along the curve, and 5 feet in the direction of the radius, were made by bolting straight, or nearly straight, castings together, and proceeding thus as in stonework, only bolting each frame voussoir to its neighbor instead of using mortar. The next cast-iron bridge of importance was Southwark Bridge, completed in 1819 by Sir John Rennie. The span of the center arch of this bridge is 248 feet; rise, 24 feet. The side arches are each 210 feet span. There are eight ribs in each span. The soffits consist of solid masses of cast iron of a depth similar to the voussoirs of a stone bridge. Each principal rib is 6 feet deep at the crown of the arch, and gradually deepens to 8 feet deep at the abutments. Of modern bridges, properly so-called, both the Rochester and Pimlico Bridges are built on the arch-girder system of large segmental castings bolted together. The most recent example that has come under the author's notice is the new bridge over the River Trent at Nottingham, from the designs of Mr. M.O. Tarbotton, M. Inst. C.E., erected in 1871. This bridge consists of three main arches of cast iron, each 100 feet span, and 10 feet rise. Each arch-rib is in three segments bolted together, and also connected to the other ribs transversely. The ribs are 3 feet deep at the springing, and 2 feet 6 inches at the crown, the section being of an I form. In conclusion it may be safely said that not one of those large span cast-iron bridges are likely to be repeated in the future. Our knowledge of cast iron has been got slowly by experience, and that experience teaches us that under circumstances combining tension and vibration it ought not to be used. On the other hand, considering the high superiority of cast iron under compression to wrought iron, and the great disparity in its favor in point of economy, it will always be largely used

under circumstances where these conditions are present, and bearing this in mind its nature and properties cannot be too well understood. It has been with this conviction on his mind that the author has been led to lay the above imperfect notes before the society on the present occasion.

THE DISPOSAL OF SEWAGE.

From "The Engineer."

For some time past little had been heard concerning the disposal of sewage. Those who at one time wrote, spoke, and lectured most earnestly on the subject, seem to have entered upon other pursuits. They have apparently abandoned their old love in favor of something new, or else they have lapsed into a condition of apathy, possibly sullen, possibly not unpleasant. There is but one way of explaining this silence. The necessity for disposing of sewage is as pressing now as it was at any time within the last dozen years, and the reason why enthusiastic projectors no longer write to the daily press; or publish books on the subject, is that all their prognostications, almost without exception, have been falsfied, while their anticipations have ended in nothing, save disappointment. At one time it was fiercely maintained that the sewage of each individual possessed a money value of about 8s. per annum. It is not impossible that men may still be found who hold that the sewage of London might be made worth about £1,500,000 a year. In order to impart this value to it, it would be necessary to avoid the use of water as a carrying agent; in a word to adopt what is technically known as the dry system. The concession that water-borne sewage was not worth 8s. per head per anum was wrung from a considerable party only after the lapse of a long period, and by the hard logic of facts. It is now, however, generally admitted that the money value of sewage is much less than it should be according to chemists. But it is still held to possess considerable value, and we now and then have this proposition urged on our attention. Mr. Mechi, for example, not long since endeavoured to turn men's minds once more to the utilization of sewage. It is rumored that the moment commercial prosperity has been sufficiently restored, more than one company will be floated with the object of making a profit out of sewage. It is not improbable, indeed, that the lull in what we may term sewage agitation is nearly at an end; and this being the case, it is worth while to call attention once more to some truths which, however they may be glossed over or distorted by enthusiasts, have certainly never been refuted.

To dispose of sewage at a profit has been tried over and over again with the utmost persistency, and at an enormous cost. The result has been complete failure. In certain isolated cases, where the quantity to be dealt with has been small, or where the conditions have been exceptionally favorable, a small profit has been made either by individuals, companies, or towns; but all attempts to get rid of the sewage of cities, and to realize for those cities even a moderate profit on the cost of the necessary works as well, have been failures. There are two ways in which sewage can be delivered on land. It can be supplied to the agriculturist either in the fluid or in the solid form. Even the most warm advocates of irrigation now admit, what we years ago maintained, namely, that fluid sewage possesses very little more value than plain water. In other words, the dry hungry soils, on which fluid sewage has heretofore been used to most advantage, could be made to produce very nearly as good crops if irrigated from a river or a spring instead of with the sewage of a town. Granting this, however, the fact remains that very good crops can be grown on land irrigated either with water or sewage, and the sewage, consequently, does possess a money value. Solid manure made from sewage is of various values. It has been sold at all prices, from 10s. to £3 per ton; as a rule it is worth probably about £1 10s. per ton. It will be seen, therefore, that those who most earnestly advocate the utilization of sewage have something to go on; unfortunately, for

them and for others, they have not enough. The causes of the failure of sewage schemes are very few and very simple. They lie in the difficulty met with in obtaining land adapted for the application of sewage; and the cost of delivery. The first-mentioned obstacle is one of overwhelming magnitude, conflicting interests warring with each other and with sanitary authorities. To find a suitable site for a sewage farm is often a work of great trouble, and the sum demanded for it is invariably very large; nor is there any help for these things. Towns do not as a rule stand on high ground, but in valleys or places close to rivers, and, of course, at the base of the watershed of a district. Thus in most cases the sewage has to be pumped up to a higher level than the town, that it may gravitate to the farm. And so it happens that by the time the farm has been obtained and the necessary machinery has been put down for pumping a mighty sum has been expended. In a few cases the sewage can be made to grow crops enough to pay the cost of pumping; but there is no known instance of a really large town obtaining in this way a return great enough to pay the cost of pumping, and a fair percentage on the capital invested besides. No doubt mistakes have in many instances been made in the selection of sites and the design of machinery; but with all allowances the fact cannot be argued away—that no example can be cited of a large town pumping its sewage on to a farm and making an adequate profit on the outlay. We believe that Mr. Bailey Denton has succeeded in obtaining a satisfactory result in a few cases on a small scale; but with small scale operations we are not now dealing. It is not possible to see how any change for the better can be made in this respect, and we fancy we do not go too far when we say that even the warmest advocates of sewage irrigation now admit that it is impossible to buy or rent a farm and pump sewage on to it at a profit. In a few instances towns have been able to sell their sewage at so much per thousand gallons to farmers, and under these conditions the loss has been small, while in others a trifling profit has been made; but on analysis it will be seen that the result has been brought about because the town has only had to pay for pumping and the interest on the cost of the works, and has not been embarrassed at all about land. In other words, if farms could be had gratis on which to spread sewage, the whole aspect of the matter would be changed for the better, and sewage irrigation might be practised at a good profit. As there is no more chance of this than there is that the sky will fall, we are justified in maintaining that from the application of fluid sewage to land towns and cities have nothing to hope in the way of emolument.

As regards the disposal of sewage in the solid form matters do not look much more hopeful. Although the pail system can be, and has been, used successfully to some extent in certain districts of a few cities and towns, it is totally opposed to the instincts of a very numerous, powerful, and refined class, who will not have it at any price. Those who undertake to dipose of sewage in the solid form must therefore count upon having to deal with it in the fluid form at some time. This has been the ruin of all manner of schemes. Sewage to be worth £2 a ton must be, comparatively speaking, quite dry. Now, it is an easy matter enough to throw down the solid constituents of towns sewage. Settling tanks of sufficient size, and a little lime, or lime and clay properly used, will soon bring about the desired result. The supernatant water can be drawn off as clarified sewage and poured into a river without fear of the consequences. But what is to be done with the enormous mass of foul mud left behind? The really valuable part of the deposit—the ammonia—does not amount to one ton in a hundred, perhaps not to one ton in a thousand. The greater portion of the mud comes from the attrition of the streets. To get the ammonia on to the land it must be taken there in company with the street sweeping, sand and gravel, of no possible value as fertilizers. The water has to be driven away from all this mass of mud, and this can only be done at a cost which raises the value of every ton of the manure far above the price which the farmer can afford to pay for it. The bankruptcy of company after company which has attempted to make a saleable dry manure

from sewage demonstrates the accuracy of our statements.

To sum up, the manurial value of sewage has been very much overestimated. The cost of obtaining such valuable constituents as it really does possess in an approximately concentrated form is much too great to enable a profit to be made. Attempts to extract the equivalent of a very inferior guano from sewage find a parallel in the struggle to get a profit out of the crushing and amalgamation of a poor gold-bearing quartz. The rock does not contain gold enough to pay for the labor spent in getting it out. In like manner, sewage does not contain ammonia enough to pay for the cost of getting it in a portable form. Our readers will do well, under the circumstances, to look with the utmost caution at the shares of all companies proposing to make a profit out of the utilization of sewage. We do not assert that it will always be impossible to get an adequate return for money spent on such schemes; but we do assert that it is impossible at present, and under existing conditions. Circumstances may be met with now and then which alter the aspect of the question. For instance, pumping may not be necessary, and land may be had for next to nothing. Then with good management, it is quite possible that a profit may be made on a moderate outlay. Again, the dry system may be in use, and as the cost of getting rid of water is avoided, a profit may be made by converting the contents of pails into *poudrette*. But these are all exceptions to the rule, and any person or company undertaking to deal with sewage must be prepared to get it in the but too familiar liquid form; and to make a profit out of this sewage, manipulate it how we may, is a work which has never yet been accomplished on a large scale. The only chance of making a profit lies in getting the sewage for nothing and suitable land at a very moderate rent—conditions which are very rarely at the disposal of a public company.

ON KEEPING IRRIGATION CANALS CLEAR OF SILT.

By Robert Burton Buckley, Assoc. M. Inst. C. E.

A Paper read before the Institution of Civil Engineers.

There are four methods by which it is possible to exclude more than a desirable proportion of silt from entering an irrigation system: (1.) By works in the river, which will clear the water before it enters the canal. (2.) By so constructing the head sluice of the canal that only water bearing the desired proportion of silt is admitted. (3.) By constructing a depositing basin near the head of, and in, the canal itself, to be cleared either by dredging or by hand labor; or, what is practically the same thing, by making two supply channels from the river to the canal, one to be used while the other is being cleansed. (4.) By constructing a double row of sluices, with a settling tank between, so arranged that the water is drawn off from the lower row carrying the desired amount of silt, and so designed that the deposit in the tank can be flushed back again into the river.

These systems are, of course, applicable under different circumstances. The first can be rarely used, and only when the local conditions are suitable. As for example, where the bed of an inundation canal is perhaps 8 feet or 10 feet above the level of the bed of the river, and which canal is therefore only supplied when the river is in flood. In such a case, if a position for the head of the canal can be selected behind an island covered with brushwood, the top of which is perhaps a little below, or even slightly above the high flood level, it may be well worth the cost to make an artificial connection between the head of the island and the main land, so that all the water entering the canal will first flow through the bay, formed between the island and the main land, entering that bay from below. The velocity of the water in the bay will thus be diminished; the water will deposit silt in the bay instead of carring it into the canal and if the bay be a large one the canal may work for many years without its bed silting up.

The same principle can be employed on large irrigation schemes, by altering the

methods now generally adopted in these works. The almost invariable arrangement is, that the weir which stretches across the river, at a height of from 8 feet to 15 feet above the bed, is cut by two sets of under sluices, which are purposely set as close as possible to the head sluices of the canal immediately above the weir; the floors of the head sluices and of the under sluices being at the same level. The under sluices are placed in this position so that silt may not accumulate in front of the entrances to the canal, and thus impede the free entrance of boats to the lock and of water to the canal. This object is attained by opening the under sluices during floods, thus drawing down a rapid stream immediately in front of the openings to the canals, which scours the channel, and removes any deposit that may have accumulated. At the same time that this action of the under sluices clears the approaches to the canal, it causes the canal to be more deeply silted, for the higher velocity produced by the scour of the under sluices removes an extra quantity of silt from the bed of the river, and it is from this rapid and silt-bearing stream, impinging directly on the head sluices, that the canal is supplied. But if the weir were constructed with a double set of under sluices at each end, one set being in the line of the weir, and about 200 feet from the river bank, and the other set some distance lower down the river, but connected to the upper set by a flank wall parallel to the river bank, and if the off-take of the canal were placed immediately above the lower set, the stream flowing to the upper set would not pass in front of the off-take to the canal. The silt-bearing water would pass through the upper set of under sluices with full velocity, while that portion of the river destined for the canal would have its velocity checked immediately opposite the flank wall, and would deposit its silt to a great extent before it reached the head sluices of the canal. To sweep away the silt which would be deposited between the weir and the head sluices, it would be necessary to close the upper under sluices and to work the lower ones. This plan would be rendered most effective by closing the head of the canal for a few hours every week, while the lower under sluices were opened, so that the channel might be kept clean without allowing any silt-bearing water to have access to the canal.

In almost all cases the head sluice of a canal is formed by rows of single shutters sliding in vertical grooves, so that water is always first admitted to the canal from below the shutters, that is at the level of the sluice floor. If the sluices were constructed so that the water was drawn from the top instead of from the bottom of the river, much less silt would be carried into the canal. In rivers which rise moderately, it is best to have a single opening in each vent, covered by three or four shutters sliding in a vertical groove; and each of these shutters should have independent opening gear. In rivers liable to floods rising 30 feet it is necessary to have, in each vent of the sluices, several openings at different levels, each opening being fitted with an independent shutter, so that water can be drawn off at different levels as the flood rises or falls. This way of dealing with the silt can at most be but partially effective; but there are some rivers, carring a small amount of silt, to which this system may be applied with sufficent effect to render the clearance of silt from the canal unnecessary.

The third method is frequently adopted in Indian canals. The first $\frac{1}{2}$ mile of the canal is excavated with a base sufficiently large to cause a great diminution of velocity; the silt is deposited during floods, and excavated when the canal is closed during the summer; or perhaps it is dredged out at a cost even more excessive than that of excavating it by hand.

The fourth method is peculiarly suitable for rivers with a rapid fall. It is also most desirable where a canal runs alongside of the river for some distance before branching off into the country. If this method be adopted, the channel for the first $\frac{1}{2}$ mile or so must be of such capacity that the velocity of the water in it, when carrying the full volume required for the canal, shall not exceed that which will allow of the deposit of the matter in suspension; so that the water, when it reaches the end of this length, shall contain only that proportion of silt which the channels below are arranged to convey to the fields. At the end of this broad channel a sluice will have to be built to carry the full discharge required in the canal with little or no head upon

it. The head sluice on the river bank must be so designed that, with only a moderate flood in the river, a sufficient quantity of water can be introduced into the canal to generate a velocity of 3 feet to 4 feet per second in the broad reach, the flushing sluices leading back from this reach to the river being arranged to discharge a corresponding quantity, or even a larger quantity, of water. These sluices might be fitted with falling shutters. The largest flushing sluice should be about 150 feet to 300 feet from the head sluice, for it is about this point that the heavy sand is deposited, and where the greatest scour would be required. This system is the most effective and the least expensive for large schemes. If the head sluice on the river bank be constructed on the principle of taking water from the surface of the river, instead of from below, the minimum amount of silt will enter the broad reach, and that can, under favorable conditions, be cleared away, by closing the sluices at the extremity of the broad channel for a short time, and opening all the lower shutters of the head sluice on the river bank and the various flushing sluices.

SANITARY FALLACIES.

An Address delivered at Croydon, by Professor W. H. CORFIELD, M. D.

From "The Builder."

SANITARY science, properly so-called, is a branch of medicine, or, perhaps, I should rather say, a sister science to pathology, for it is the science which studies the causes of diseases, and its place among the sciences is between those of physiology,—the science of life,—and pathology,—the science of disease. We see, therefore, how it is that sanitary science, or hygiene, could only become a science in quite recent times, as it was impossible that it should be scientifically studied until physiology and pathology, upon which it is based, became scientific themselves. The more a branch of knowledge approaches to the character of a true science the more readily are fallacies detected. Although even in the highest science, the most certain branch of human knowledge, mathematics, in connection with which one would think no fallacies could exist, there are still to be found keeping their hold upon the minds of a certain class of investigators, as witnesses,—the supporters of the theory that the earth is flat and that the sun goes round it, the circle-squarers, and the searchers after perpetual motion. If in the highest and most perfect science the power of fallacies does not cease to exist, can it be wondered at that in the youngest, which I will not, however, call the most imperfect, although fallacies which reigned triumphantly while it was yet only an art—the art of preserving the health—and before it became really worthy to be dignified by the name of a science, have been exposed, there are still many others which have a certain, and, in some instances, a most important influence upon the mind of large masses of the community—an influence necessarily for evil? On the other hand, I must point out at once that what is necessary and inevitable in one generation, or at one period of time, may be a mischievous fallacy at a future period and in an advanced state of knowledge . . . . . .

This grand fallacy, the mistaken union of theology and medicine, continued through Mediæval times; and as late as the year 1511, Henry VIII. ordered that physicians and surgeons should be examined by a bishop or vicar-general, with the assistance, it is true, of "such expert persons as they shall think desirable," while the power of granting the degree of Doctor of Medicine remained in the hands of certain high ecclesiastical dignitaries, to a much later period, even if it does not nominally exist now. Through all these dark ages, when the principles of preventive medicine laid down by Hippocrates, Galen, and Celsus were unknown to the multitude, and untaught and unpractised by those whose business it was to teach and practise them—when (more shame to them still) the regulations laid down in

that book of which they were the jealous guardians, to which they alone had access, of which they proclaimed themselves the expounders and the teachers, were neglected as completely as if they had never been ordained—filth reigned supreme, the dirty houses were crowded together in narrow streets and courts; the rushes which formed a carpet for the floors were never removed, but piled layer on layer, forming a series of filthy strata often many years old; no attempt was made to check the spread of infectious diseases by the isolation of the sick or by any of the other methods prescribed by Moses; and what was the result? In those ages, and the succeeding ones—the partakers too in the results brought about by this lamentable and gigantic fallacy—plagues held triumphant sway. In the fourteenth century, the Black Death, after traveling over the eastern part of the Old World, reached Europe, and soon arrived in England. It spread over the whole country, and caused such a frightful mortality, that only a tenth of the inhabitants are believed to have remained alive, while "Europe is supposed to have lost an aggregate of 40,000,000" (Dr. Guy). As I have pointed out elsewhere, the only people whom this disease seemed to spare, were those who, however imperfectly, followed the regulations prescribed by Moses, the Jews, whose immunity was so marked that they were accused of spreading the disease by poisoning the water, and were burnt alive by thousands in various parts of Europe. The Black Death reappeared as the Oriental Plague during the sixteenth and seventeenth centuries, and the last time that it appeared in England, in the year 1665, it killed between 70,000 and 80,000 persons in London alone.

But besides the Oriental Plague, a frightful prevalence of other diseases, some of which, as the "sweating sickness" are now unknown; while others, as typhus, scurvy, influenza, dysentery, cholera, and even small-pox, have lost much of their terror, must be included among the consequences of the fallacy which had overspread the world. This fallacy was removed by the gradual divorce of medicine and theology; and the seventeenth century, which had seen the last of the Oriental Plague as far as England was concerned, saw Anatomy raised to the position of a science, by the labors of Vesalius, of Eustachius, of Fallopius, of Malphighi, of Glisson, of Sylvius, of Willis, and of others, almost all of whose names are worthily preserved forever in the names given to various parts of the body, and saw physiology receive the grand impetus given to it by the discovery of the circulation of the blood by William Harvey, and scientific chemistry begin gradually to emerge from the Arabian alchemy.

Thus began again the reign of rational medicine, and from that time to this, the study of methods for the prevention of diseases has been pursued, and in many instances with remarkable success. But although we have got again into the right path, there is, as may be expected, seeing the short time that we have been in it, a vast amount of ignorance prevailing in connection even with the rudimentary principles of sanitary science, and the ignorant multitude are too often led astray by specious fallacies, propounded with some show of reason, and often with great bombast, by persons who have no right to speak with authority on such matters at all, and who are at best "blind leaders of the blind"; but this, we may rest assured, will always be the case, as is shown by the example of mathematical science that I have already instanced. All that we can do, therefore, is to point out such fallacies as they arise, and to warn those who are in danger of being misled by them.

Against all sanitary improvements whatever we find one argument continually brought—that things have gone on in the same way for many years, and there is no reason why they should be changed; and that our forefathers from generation to generation lived under sanitary conditions, and why should we not do the same? That cholera, or enteric fever, or diphtheria has never broken out in a place, or in a particular house, and so it need not be expected! Such are the forms in which this argument meets us at every turn; but those who use it forget that our forefathers died in those places; they forget that in all places which have been made cleaner, from which refuse matters have been

removed more speedily, where overcrowding has been abated, where more efficient drainage arrangements have been carried out, the general death-rate has been lowered. When they say that because such a disease as enteric fever has not appeared in a place, therefore it never will, they forget that when cholera or enteric fever is introduced into a place where the conditions are favorable for its spread, where the air is tainted and the water-supply rendered impure with excremental pollution—that in that place, although such diseases may have been absent for so long that their existence has been almost forgotten, they will spread like wildfire and decimate the population. They forget, in fact, that people who are living in the midst of general unsanitary conditions are in a worse plight than people living in the crater of an extinct volcano, for not only may any one of the severest epidemic diseases break out among them at any time, but they are continually sacrificing unnecessary victims to the demon filth. I have mentioned some of the communicable fevers. Now what I believe to be an important fallacy still exists in connection with the poisons of these diseases.

It was formerly thought, and was maintained by Trousseau, that the poisons of these diseases might originate anywhere, at any time under suitable conditions—the specious argument being that having arisen somewhere, at some time or another, there is no reason why they should not originate anywhere or at any time. Without entering into the vexed question of the nature of the poison of such diseases, I will merely point out that this belief is now almost universally scouted with regard to the majority of such diseases. How many persons are there who believe that smallpox or scarlet fever, measles or whooping-cough, arise independently of previous cases of these diseases? And yet we find not a few, supported by the weight of great authority, who believe in the spontaneous origination of the poisons of typhus and enteric fevers, of diphtheria and of cholera. The arguments brought forward to support this position are most of them fallacious in the extreme, and I am bound to say that the arguments advanced to prove the *de novo* origination of the poison of enteric fever are of themselves sufficient to render it in the highest degree improbable. They are, indeed, so weak that no one really capable of judging the value of a scientific argument could from them come to any other conclusion than that the position was untenable. But a practical and very serious mischief has arisen from the spread of these doctrines. We are told that enteric fever is not contagious, and we are told distinctly in so many words that it is rarely, if ever, communicated from person to person; we are told that in the great majority of instances the poison of this disease originates *de novo* in decomposing excremental filth; we are told that the intestinal discharges of patients suffering from this disease do not contain the poison of the disease, although they may be more prone to the special decomposition by which the poison is produced, and the result of all this is, that a large number (I will not say the majority, for I hope it is not so) of the medical practitioners throughout the country take no pains to destroy the poison of this disease at its source—the virus-laden discharges of the intestinal canal. It might be thought that after people were told that living under bad conditions as regards the removal of filth would engender enteric fever among them, they would be even more careful to prevent the possibility of its appearance than if they were told that it would certainly spread if brought to them while living under such conditions; but this is not so, and for the simple reason that the people know well enough that enteric fever does not arise under these conditions. They may be deceived about the general death-rate, but they know perfectly well that a field may have the richest possible soil, may be well-manured and well-watered, but that no wheat will grow in it unless the seed is sown—that a place may be in the most unsanitary condition conceivable for many years, and that enteric fever will not spring up in it. And when they are told that it will, they do not recognize this as a fallacy, but jump to the conclusion that the whole of sanitary science is a philosophical fancy, not worthy the attention of practical people.

But there is still a great fallacy abroad

in connection with the question of the removal of refuse matters from the vicinity of habitations. People talk and write as if the water-carriage system and the conservancy system stood upon the same footing—the principle of the one being the *immediate* removal of excretal matters from houses, and that of all the others being, as their name indicates, the keeping of such matters in and about the house for a certain time. The one is a correct principle, the other is a false one; and it is no argument at all to say that where the water-carriage system is badly carried out, the result may be worse than where the conservancy system is carefully managed. In sanitary matters, as well as in everything else, we should follow correct principles. If we do not, but by arguments equally specious and fallacious try to persuade ourselves that "practically speaking" (according to the cant phraseology of the day) better results may be obtained by following false principles, nothing is more certain than that by an inexorable law of nature true principles will assert their position, and we shall be punished for our mistake by being landed in difficulties greater than we had to contend with at the outset. It is a very old and often-exposed fallacy to argue against the use of a thing from the abuse of it, and to argue against the water-carriage system because when surface-drains have been called upon to do the duty of sewers, for which they were not intended, and of which they are not capable, or because the sewage has been turned into the water-courses, which have thus become unfit to supply water for domestic purposes, is an excellent example of this kind of fallacy. I do not say that a well-managed conservancy system is not better than a badly-managed one, nor far better than no system at all, nor do I say that there are not places where the difficulty of carrying out a water-carriage system are not so great as to be almost, if not quite, insurmountable; but I do say that in towns where a water-carriage system is possible, there is no room for choice in the matter. The mischiefs that have been traced to the water-carriage system have occurred from the abuse of it, and not from the proper use of it. Sewer air, about which so much has been written, is injurious when it is collected in badly-ventilated sewers, and allowed to escape from them into the houses; but in an impervious sewer with a proper fall, sufficiently flushed and efficiently ventilated, the noxious ingredients of sewer air are scarcely formed at all, and the air of the sewer is hardly appreciably different from that in the street, while its foulness bears no comparison to that of the atmosphere of many inhabited rooms. The proper way to ventilate sewers is to have a sufficient number of openings leading into them from the surface of the roads, as has been demonstrated over and over again, but I see that the ridiculous practice of having, as far as possible, air-tight sewers, and connecting them with the flues of furnaces, notwithstanding that the fallacy of it was exposed by the Health of Towns Commissioners in 1843, and has been pointed out over and over again ever since, still has its advocates. The commissioners pointed out that in the first place the action of the furnaces was at times so strong as to draw all the water out of the traps on the house-drains, and at other times so ineffectual that the air from the sewers was drawn into the houses through the unsealed traps. They pointed out, too, that in a case where some of the sewers in Battersea had been connected with the furnace of some soap-works, on one occasion coal-gas escaped from a main into the sewer (as has frequently happened since, and not so long ago in the neighborhood of Great George Street, Westminster) and an explosion occurred which blew the works to pieces.

Another important matter in which we are liable to be led astray by false principles, is that of the supply of water for domestic purposes. A man deservedly eminent in his own branch of medicine, told the public not so long ago, from a position that lent weight to his words, that water-analysts and medical officers of health had all gone wrong about water; that the small quantities of organic matter that were discovered in water were matters of no importance at all, and that all water, however pure it was, was contaminated with organic matter as soon as it got into our mouths; that the greater part of our food consisted of organic matter, and that it was ridiculous to condemn a drinking water

because it contained small quantities of organic matters. The obvious fallacy of such arguments must be patent to all who have thought upon the subject at all, but to the multitude who allow others to think for them, such fallacies, coming from the mouth of one whose words were entitled to be listened to with respect, were calculated to do a vast amount of mischief. There are organic matters and *organic matters*, and it is not because beef and mutton are good for food that putrifying filth, in however small a quantity, coming from sources likely to be tainted with the poisons of specific diseases, is to be tolerated in water for domestic use; and this leads me to speak of a still greater fallacy in connection with the water supply. We are told that it is not necessary to go to the purest sources for water; we are told that we may take a water that has been once polluted, filter it and give it to the people to drink, that it is a "practically wholesome" water, that no harm can be shown to have resulted from it, and so forth, and we are given averages of its composition to prove that it is "reasonably pure" to be used; but it is not averages we want—we want to know the quality of the worst samples that are supplied. It is ridiculous to tell a man that the average quality of the water given him to drink is good, if on one day in the year he gets water that is "quite unfit for dietetic purposes." But the people are awakening to this matter. They will not be put off by such specious arguments and fallacious reasonings, they will insist on the "practical" carrying into effect of the true principle as laid down by Mr. John Simon.—"It ought to be an absolute condition for a public water supply that it should be uncontaminable by drainage." . . . . .

Although I do not mean to enter in a statistical discussion, I will mention one or two serious statistical fallacies that are very prevalent, and out of which much capital is made. We are told that in spite of sanitary improvements, the death-rate remains the same; now, considering that "the mortality of the City of London was at the rate of 80 per 1,000 in the latter half of the seventeenth century, and 50 in the eighteenth, against 24 in the present day" (Farr), this statement seems rather audacious. We are also told that the death-rate of London is and has been for some time practically stationary, but since the density of the population is increasing, the death-rate ought to be increasing, whereas it is actually diminishing. Dr. Farr shows that the death-rate of London (calculating from its density) ought to be 35.2 per 1,000 per annum, whereas it is now under 23. Again, we are told that the death-rate from zymotic diseases is stationary; but surely the wonder is that it is not increasing rapidly.

Yet another statistical fallacy: the death-rate of London is very low indeed; we are positively told that this is due to the influx of healthy lives from the country! whereas, as a matter of fact, they make an almost inappreciable difference in the death-rate. The annual influx of immigrants forms in time a permanent addition to the population, but as their death-rate (say that of persons over twenty years of age) differs but little from that of the community at large, or from that of persons under twenty years of age, they scarcely affect the general death-rate themselves at all; if we are required to debit London with the deaths of persons under twenty years of age, of whom the immigrants may be said to be the survivors, we must also credit the population of London with the additional population, under twenty years of age, which would result from an annual number of births equal to that of the immigrants, and of the persons under twenty whose deaths we have taken into account. Thus it can be easily shown that the death-rate is hardly affected at all by immigration.

M. Soret, who pointed out that cerium sulphate, with the aid of the induction spark, exhibited a beautiful violet fluorescence, has continued his researches in the same direction, and finds that in the same manner the solutions of the salts of many earthy metals exhibit the same phenomena. In this category he enumerates lanthanium chloride; didymium chloride and sulphate; terbium, yttrium, erbium, ytterbium chlorides; philippium chloride; thoxium sulphate; zirconium chloride and sulphate; and aluminum and glucinium chlorides.—*The Engineer.*

Fig. 1.

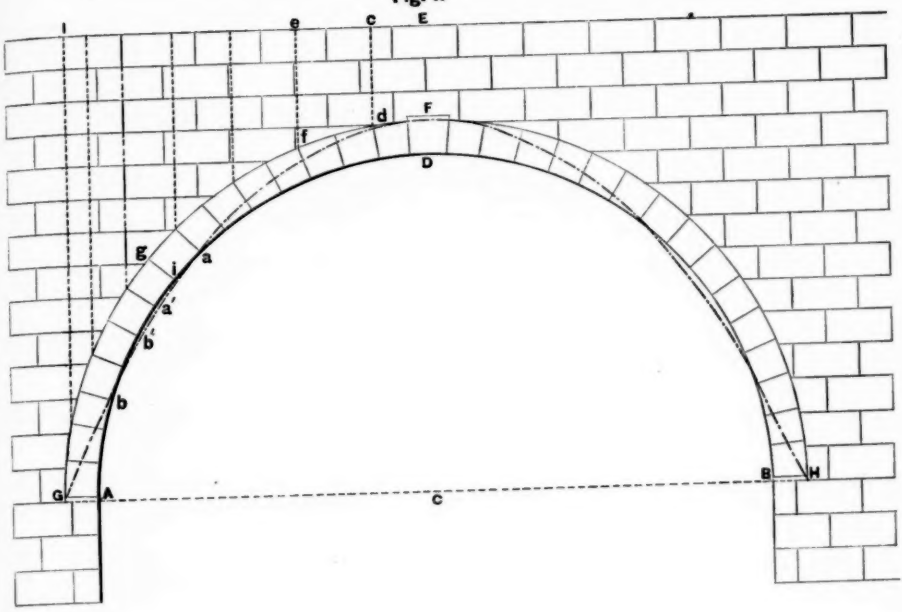


Fig. 5.

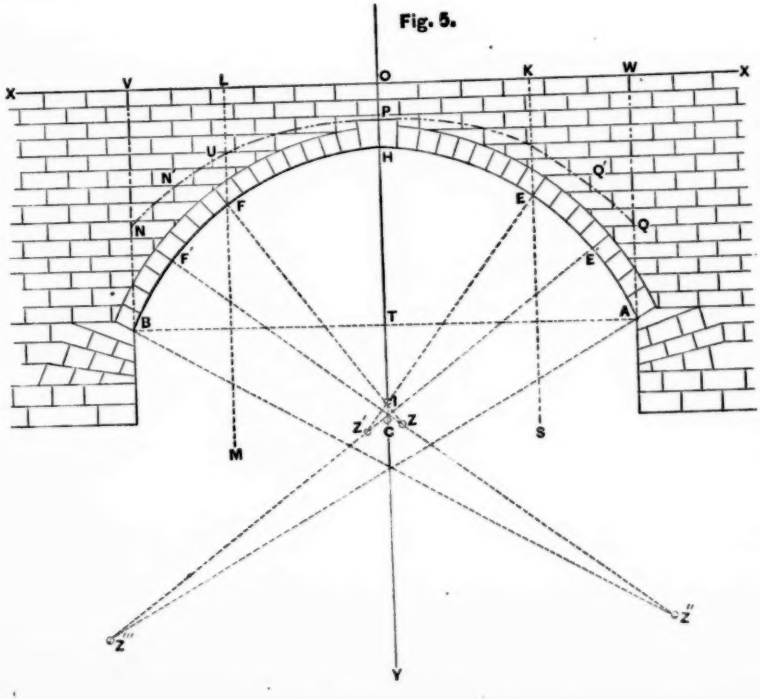


Fig. 2.

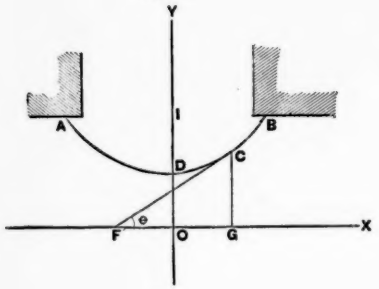


Fig. 3.

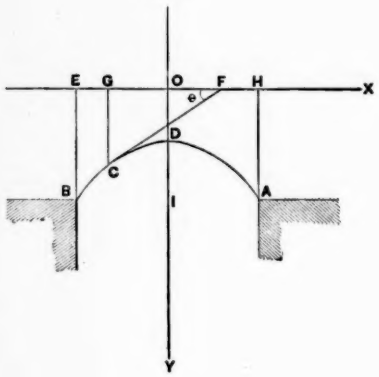
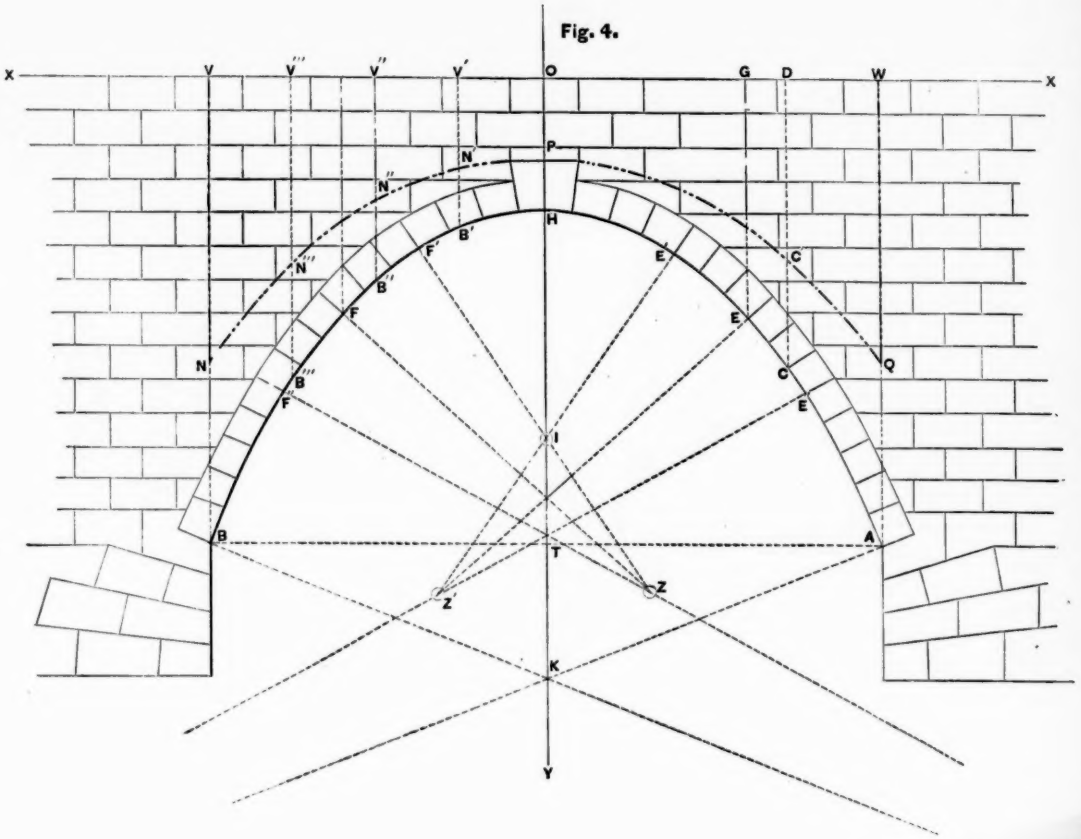


Fig. 4.



ARCH BRIDGES.

By William H. Baker, Civil Engineer.

Some time ago the writer had occasion to investigate the principles upon which the proper construction of the arch, as used in bridges of masonry, depends, and in doing this evolved some facts, which, although well-known to mathematicians, have not perhaps been so generally brought to the notice of engineers in practice. The object of this paper, therefore, is to set forth those facts as clearly and as concisely as possible, with, however, such mathematical deductions as will enable them to be clearly understood, and afford those engineers who are acquainted with higher mathematics an opportunity to examine the accuracy of the reasoning.

The usual form of the intrados or soffit, of a stone arch bridge is circular, the longitudinal section being either a semicircle or a smaller segment.

We will take as an example a full semicircular arch as represented in Fig. 1, having the following dimensions: Let the span, AB, be 40 feet, the rise, CD, 20 feet, and the distance from the crown of the arch to the road bed, DE, 8 feet. These are the data which the engineer generally has from which to design his bridge. The first thing to be determined is the thickness of the arch stones or voussoirs. An empirical rule for the depth of the keystone, which has been used in many designs, is represented by the formula:

$$\text{Depth of keystone} = \tfrac{4.1}{100}\sqrt{\text{radius of arch at crown.}}$$

This formula has been applied, with slight variation, to some of the largest arch bridge designs in the world, notably the Grosvenor bridge over the Dee, having a span of 200 feet, the London Bridge and the Dean Bridge near Edinburgh. By applying this rule to our example, we find that it gives for the depth of the keystone 1.83 feet. The other arch stones are generally somewhat thinner than the keystone, and it would be fair to presume that if this rule were strictly adhered to in a bridge of the above dimensions, the voussoirs, excepting the keystone, would not have a thickness of more than 1.75 feet. Let us, however, for safety, give all the arch stones a thickness of two feet, increasing the depth of the keystone in the same proportion.

This is probably a fair example of the stone arch bridges throughout the country, and it is now to be seen what the nature of the forces, acting among the arch stones, are. We will suppose the bridge is to be composed of granite, weighing 170 lbs. per cubic foot. The horizontal thrust at the crown of the arch in this case, will be about 19240 lbs. per foot of width of the arch. This can be obtained by finding the center of gravity of the mass, EDAGI, through which its weight acts, tending to turn it about the point, G, which moment is resisted by the horizontal thrust, acting through the point, F. That is: Calling W the weight of the mass, EDAGI, and p the perpendicular distance of its line of action from G; H, the horizontal thrust, we have: $Wp = 22$ H. (22 feet being the distance to CF.) $\therefore$ $H = \frac{Wp}{22}$

Considering, now, this horizonaal thrust to act through the point, F, by combining it with the weights of those sections of EDAGI, into which this mass may be divided by any vertical planes, *cd*, *ef*, etc., by the ordinary principles of the composition and resolution of forces we obtain the line, HFG, which is the *Line of Pressures* of the Arch. Wherever this line cuts the joints of the arch stones are the points which receive the pressure of the whole mass of material above. It will be seen, therefore, how important it is that these points should be as near the center of the joints as possible, and it will also be seen by consulting the Fig. how few are the joints at which this point of pressure is near the center.

Indeed, from *a* to *b*, the line of pressures passes entirely outside the arch stones; in other words, the ring course of masonry is not thick enough to contain the line of pressures. What is the result? As soon as the line of press-

ures passes outside of the arch stones at *a*, it creates a moment, tending to turn the stone *a*, *g*, about the point, *i*, and, consequently, to open the joint, *g*, *i*, at *g*. It, also, by pressing upon the point, *i*, tends to move the stone, *g*, *a'* in a horizontal direction, which movement is resisted by the mass of material always built upon the "hip" of the arch, and equilibrium takes place, with, however, the pressure of the whole mass of material above the joint, *g*, *i*, resisted at the point *i*.

The same thing occurs at the next joint, and the next, until finally the line of pressures re-enters the arch, and terminates at the point, G. This tendency of the joints at the "hip" of the arch, to open at their outside extremities, although resisted so far as to prevent the fall of the arch, is not entirely removed.

An examination of almost any arch bridge will show a slight opening of these joints at their outer extremities and an immense pressure at their inner extremities. The result is that the stone gradually becomes crushed at these latter points and the consequent settling and distortion of the bridge follows. If the ring-course is laid in cement, the cement becomes cracked, water is allowed to get in, and in the winter the bridge is further injured by the action of frost. The same difficulty occurs at the crown of arch, DF, and at the springing, AG, although here the tendency to open is upon the inside, and the tendency to crush upon the outside.

The next question to be considered is: What is the remedy? It certainly is not in using elliptical arches, with the longer axis for the span, as that would evidently increase the difficulty, the form of the curve of pressure being very different from an ellipse. Elliptical arches are often used for bridges, but always at the expense of strength, and sometimes in such a manner as to make the bridge unsafe. One remedy might consist, perhaps, in increasing the thickness of the arch-stones such an amount that the line of pressures could be drawn within the middle third of the joints. It will be seen by Fig. 1 that this line falls outside the arch about 4 inches. This remedy would require, therefore, in this case, the thickness of the arch-stones to be 7 feet, or more than one-third the rise of the arch. Such a thickness is probably impracticable, as no engineer would think of putting such a mass of material into the ring course. Prof. Rankine says that the stability of an arch bridge in which the line of pressures cannot be drawn within the middle third of the joints is very precarious. Yet to, probably, none of the semicircular or elliptical arches, as used in bridges, can this be done. Still these bridges stand and will, very likely, continue to stand, as the large amount of "backing" put upon their "haunches" keep the voussoirs in place. The whole tendency of the bridge, however, is to fall, and certainly such a condition of things, from an engineering point of view, is very bad. Any bungler can put together a lot of wedge-shaped stones, and cover the whole with masonry, in such a manner that the structure shall not *fall*, but the duty of an engineer is to so unite the component parts of the structure that the whole tendency shall be to *stand*, making each joint as nearly as possible perpendicular to the direction of the pressure upon it, and so arranging the surface at each joint that it may receive and resist this pressure to the best advantage.

What, then, must be the form of the soffit of an arch bridge that these conditions may be fulfilled? It remains for us to investigate this problem, and in doing so it will be best to begin at the fundamental principles upon which the proper construction of the arch depends, viz.; The principles of the catenarian curve.

Referring, now, to Fig. 2. let us suppose ADB to represent a thoroughly supple cord or chain hanging from the two points of support, A and B, both being at the same level. This curve is the ordinary catenary, the well-known equation of which, referred to the rectangular axes, OX and OY, is:

$$y=\frac{m}{2}\left(\mathrm{E}^{\frac{x}{m}}+\mathrm{E}^{-\frac{x}{m}}\right) \quad \ldots \quad (1)$$

in which *m*, the *parameter* of the curve, is the distance OD, and E is the base of the Naperian system of logarithms, 2.71828. Representing by S any portion of the curve as DC, we also have the following well-known equation:

$$S\frac{m}{2}\left(E^{\frac{x}{m}}-E^{-\frac{x}{m}}\right) \ldots . \quad (2)$$

Let C be any point upon the curve, the co-ordinates of which are:

$$OG=x \text{ and } CG=y.$$

We will now find an expression for the area of the segment contained between the axis of X and the curve, viz., DOGC. Letting A represent this area we have:

$$A=\int_o^x y\,dx=\frac{m}{2}\int_o^x\left(E^{\frac{x}{m}}+E^{-\frac{x}{m}}\right)$$

$$dx=\frac{m^2}{2}\left(E^{\frac{x}{m}}-E^{-\frac{x}{m}}\right)$$

but by equation (2)

$$S=\frac{m}{2}\left(E^{\frac{x}{m}}-E^{-\frac{x}{m}}\right)$$

therefore $A=sm \ldots (3)$; in other words, the area of any segment, DOGC, is *slightly proportional to the arc* DC. If, now, p represent the weight of an unit of length of the cord ADB, the whole weights, W, between D and any point C, will be: $W=ps$, which is also directly proportional to the arc s; hence the following important principle is deduced, viz.: The catenary is the curve of equilibrium, and therefore the proper form for the soffit of an arch bridge composed of homogeneous material, the horizontal road bed of which is at a distance m above the crown of the arch.

Our next step will be to find the value of m. We will now refer to Fig. 3, which is the same as Fig. 2, inverted to conform to the conditions of a bridge. The given data in any particular case may be the half-span, $BI=OE=x_1$ and the vertical distance of the crown above the springing,

$DI=h=OI-OD=y_1-m \therefore y_1=h+m$,

when we shall have by putting these values of x_1 and y_1 into equation (1):

$$h+m=\frac{m}{2}\left(E^{\frac{x_1}{m}}+E^{-\frac{x_1}{m}}\right)$$

when m can be found by a series of approximations, which, added to h will give the distance of the road-bed above the springing.

Or, we may have for data the half span, x_1 and the vertical distance from the road-bed to the springing, y_1, when we have:

$$y_1=\frac{m}{2}\left(E^{\frac{x_1}{m}}+E^{-\frac{x_1}{m}}\right);$$

m can now be found as before and will be the vertical distance from the road-bed down to the crown of the arch. It may be mentioned here that

$$E^{\frac{x_1}{m}}=10^{.4343\frac{x_1}{m}}, \text{ or } \frac{.4343x_1}{m}$$

will be the common logarithm of some number to be found by consulting a table. The above case however, can rarely be used in practice as the conditions are generally too contracted. For instance, the half-span of the arch, x_1, being fixed by the nature of the ground, and the rise, h, being determined by the amount of water or other way required, the height of the road-bed above the springing, y_1, becomes a mathematical deduction and would generally be greater or less than would be practicable. If, however, the span were not fixed by local conditions, we can deduce from our former equation,

$$h+m=\frac{m}{2}\left(E^{\frac{x_1}{m}}+E^{-\frac{x_1}{m}}\right)$$

the value of x_1, the only unknown quantity, as follows:

$$x_1=2.3m\log.\left\{\frac{h+m}{m}+\sqrt{\left(\frac{h+m}{m}\right)^2-1}\right\}$$

By applying this formula to our example, that is: making $m=8$ feet and $h=20$ feet, we find the half-span, x_1, to be 15.4 feet, instead of 20 feet, as in the semicircular arch. This case would, probably, seldom be used unless a great height and short span were required. It has all the elements of strength but the conditions are still too contracted.

The next step in our problem becomes, therefore, to find the nature of the curve of equilibrium for the soffit of a bridge of homogeneous material, when the half-span, x_1, the rise of the arch, h, and the vertical distance of the road-bed above the crown, $y_1-h=a$, are all fixed by local conditions. We will still refer to Fig. 3, remembering that in this case,

as in the last, the intensity of the weight upon the curve is directly proportional to the ordinate, y. Hence the whole weight upon any portion of the curve, DC, is: area ODCG$\times w$=Aw, where A represents that area, and w the weight of a cubic foot of the material composing the bridge. Let θ represent the inclination of the curve at any point, C, to the horizon. In the figure θ=angle CFG. Let, now, the constant horizontal thrust of the arch, H, be represented by the weight of a certain amount, n^2, of the material of which it is composed, n being the length of the side of the block, its depth, in a direction perpendicular to the plane of the paper, being unity. So that—

$H=wn^2 \therefore \frac{H}{w}=n^2$. Let W be the weight upon any part of the curve DC $\therefore W=w$A.

We now have

$$A=\int_o^x y\,dx \therefore \frac{dA}{dx}=y \text{ and } \frac{d^2A}{dx^2}=\frac{dy}{dx}.$$

But

$$\frac{dy}{dx}=\tan\theta=\frac{W}{H}=\frac{wA}{wn^2}=\frac{A}{n^2} \therefore \frac{d^2A}{dx^2}=\frac{A}{n^2} \ldots (4)$$

Integrating this expression we have

$$A=B\left\{E^{\frac{x}{n}}-E^{-\frac{x}{n}}\right\} \ldots . (5)$$

B being a constant the value of which may be found as follows: Differentiating (5) we have:

$$\frac{dA}{dx}=\frac{B}{n}\left\{E^{\frac{x}{n}}+E^{-\frac{n}{n}}\right\}; \text{ but } \frac{dA}{dx}=y$$

hence

$$y=\frac{B}{n}\left\{E^{\frac{x}{n}}+E^{-\frac{x}{n}}\right\}.$$

But, from the nature of the problem, when $x=o$ $y=a$, therefore

$$a=\frac{2B}{n} \therefore B=\frac{an}{2} \quad \text{and}$$

$$y=\frac{a}{2}\left\{E^{\frac{x}{n}}+E^{-\frac{x}{n}}\right\} \ldots . (6)$$

which is the equation of the proper curve for the soffit of an arch bridge subjected to the above conditions.

In applying this formula to any particular case the only quantity contained therein, which, thus far, is not known, is n. Its value may be found by the remaining condition of the problem, viz: that the half span, x_1, and the height of the road-bed above the springing, y_1, are fixed and known. We proceed as follows: x_1 and y_1 being points upon the curve we have from (6):

$$y_1=\frac{a}{2}\left(E^{\frac{x_1}{n}}+E^{-\frac{x_1}{n}}\right)$$

from which, by reduction, we obtain

$$n=\frac{x_1}{2.3\log.\left(\frac{y_1}{a}+\sqrt{\frac{y_1^2}{a^2}-1}\right)} \ldots (7)$$

Let us apply these formulæ to our particular case. We will now refer to Fig. 4. We have for data as before: Half span=AT=x_1=20 feet. Rise=TH=y_1-a=20 feet. Height of road-bed, VW, above crown of arch=OH=a=8 feet. In this case we will suppose the whole material of the bridge, from the soffit to the road-bed throughout, to be stone. From equation (7) we have, by putting in the above values of a, x_1 and y_1:

$$n=\frac{20}{2.3\log.\left\{\frac{28}{8}+\sqrt{\frac{784}{64}-1}\right\}}$$

$$=10.39 \text{ feet.}$$

Therefore by equation (6) we have

$$y=4\left(E^{\frac{x}{10.39}}+E^{-\frac{x}{10.39}}\right)$$

$$=4\left(10^{\frac{.4343x}{10.39}}+10^{-\frac{.4343x}{10.39}}\right)$$

$$=4\left(10^{.0418x}+10^{-.0418x}\right) . \quad (6A)$$

By inserting in this equation different values of x from O, in either direction along the road bed, we obtain corresponding values for y, or the distances from the road bed down to the curve of the intrados. In this way the curve AHB is obtained, which is the proper form for the soffit of a bridge of the above dimensions, composed of homogeneous material.

In practice, however, arch bridges are not usually composed of homogeneous material. The arch stones are, perhaps,

composed of granite, and the face spandril walls of the arch built to the level of the road bed of the same material. The space between these walls is usually filled with gravel or broken stones.

The question now arises: In what proportion can these two kinds of material be used together in order to still retain the curve, AHB, as the curve of the equilibrium ?

By inspection of formula (6) it will be seen that the weight of the homogeneous material does not enter into it. Therefore, the form of the curve of equilibrium is independent of the actual weight of the material composing the bridge, it being only necessary that the material should be homogeneous. The curve would be the same, were the bridge composed of the lightest wood, or of the heaviest granite. Therefore w, representing the weight per cubic foot of this ideal homogeneous material, the intensity of the weight upon end point, C, of the intrudos, is: Wy, y being the ordinate, DC. If, now, the distance from D to C, be composed of two kinds of material, one heavier than w, and the other lighter, so that the sum of their weights upon C remains the same as before, the conditions of equilibrium will not be changed. In other words the intensity of the weight upon each point of the curve remains the same. Therefore, if w' be the weight per cubic foot of a material heavier than w, and y' be the amount, CC′ of that material used at any point, C; and w'' be the weight per cubic foot of a material lighter than w, y'' being the amount of DC′, of that material used at the same point, we have:

$$wy = W'y' + w''y'' \quad (8)$$

and

$$y' + y'' = (9)$$

when the road bed is to be level. Referring, now, to our particular case, we will suppose the thickness of the arch stones has been fixed at two feet, excepting the keystone, which is to be three feet in depth. We will suppose those arch stones and a certain distance above them to to be composed of granite of the weight: $W' = 170$ lbs. per cubic foot. The remainder of the space between the face walls to be filled with gravel of the weight: $w'' = 110$ lbs. per cubic foot. To find the line between those two kinds of material, so that the curve of equilibrium shall be the same as at the faces where the loading is composed entirely of granite. Beginning at the crown of the arch, the keystone, HP, is to be three feet in depth, hence, at this point $y' = 3$, and $y'' = \text{OP} = 8 - 3 = 5$. $\text{Y} = \text{OH} = 8$. $\therefore$ by (8) $8w = 3 \times 170 + 5x110 = 1060$. $\therefore$ W $= 132.5$.

AT THE SPRINGING.

Here $y = 28$, therefore, by (9) $y'' = 28 - y'$ and by (8)

$$28 \times 132.5 = 170y' + 3080 - 110y'$$

$$\therefore y' = 10\tfrac{1}{2} \text{ feet},$$

which is the amount of stone BN = AQ to be used as the springing, the remainder, VN = WQ, being gravel. Let us now take the points B′, B″ and B‴, where x equals respectively 5, 10 and 15 feet. The corresponding values of y are:

B′V′ = 9; B″V″ = 12; B‴V‴ = 18.

Hence by equations (8) and (9) we obtain

B′N′ = $3\frac{1}{2}$; B″N″ = $4\frac{5}{6}$; and B‴N‴ = $6\frac{3}{4}$.

In this way the curve NPQ is constructed for the *upper limit of the masonry*, upon which the gravel is to be placed. It will be seen that this curve is simply transformed from the original curve, AHB, by making each ordinate

$$y'' = \frac{\text{OP}}{\text{OH}} \times y = \tfrac{5}{8}y,$$

that is,

V′N′ = $\frac{5}{8}$V′B′; V″N″ = $\frac{5}{8}$V″B″ &c., &c

Calling this ratio $\frac{\text{OP}}{\text{OH}}$ p we have from (6)

$$y'' = py = \frac{pa}{2}\left(\mathrm{E}^{\frac{x}{a}} + \mathrm{E}^{-\frac{x}{a}}\right) \ldots (10)$$

which is the equation of the curve NPQ, by which it can always be constructed.

In the above investigation we have shown the form of the soffit of an arch bridge which should be used in order to give it the greatest strength possible. It will be seen that the curve is not an ungraceful one and would probably be considered, by most persons, to possess fully as much architectural beauty as the semicircle. One objection however, to this form of curve might suggest itself in the constant change of curvature and the consequent liability to error upon the part of those intrusted with the cutting of the stones, and perhaps, also, upon the part of those who are to calculate

and furnish the dimensions to which these stones are to conform. In order to remedy this evil we will now show that a *five-center arch* may be used, instead of the exact curve represented by formula (6), which will so nearly conform to the latter curve as to practically possess all its advantages and virtues as to strength.

The first step will be to find the radius of curvature of AHB for a few points along the curve. Let r represent this radius. The general expression for the radius of curvature of any curve is, by the differential calculus:

$$r=\frac{(dx^2+dy^2)^{\frac{3}{2}}}{dx\,d^2y} \quad . \quad . \quad . \quad . \quad (11)$$

The equation of the curve AHB is—(6):

$$y=\frac{a}{2}\left(\mathrm{E}^{\frac{x}{n}}+\mathrm{E}^{-\frac{x}{n}}\right).$$

Therefore:

$$dy=\frac{a}{2n}\left(\mathrm{E}^{\frac{x}{n}}-\mathrm{E}^{-\frac{x}{n}}\right)dx$$

$$dy^2=\frac{a^2}{4n^2}\left(\mathrm{E}^{\frac{2x}{n}}+\mathrm{E}^{-\frac{2x}{n}}-2\right)dx^2.$$

$$d^2y=\frac{a}{2n^2}\left(\mathrm{E}^{\frac{x}{n}}+\mathrm{E}^{-\frac{x}{n}}\right)dx^2.$$

$$(dx^2+dy^2)^{\frac{3}{2}}=\left\{\frac{a^2}{4n^2}\left(\mathrm{E}^{\frac{2x}{n}}+\mathrm{E}^{-\frac{2x}{n}}-2\right)+1\right\}^{\frac{3}{2}}dx^3$$

$$\therefore r=\frac{\left\{\frac{a^2}{4n^2}\left(\mathrm{E}^{\frac{2x}{n}}+\mathrm{E}^{-\frac{2x}{n}}-2\right)+1\right\}^{\frac{3}{2}}}{\frac{a}{2n^2}\left(\mathrm{E}^{\frac{x}{n}}+\mathrm{E}^{-\frac{x}{n}}\right)} \quad . \quad (12)$$

which is the expression for the radius of curvature at any point x of the curve AHB. At the crown of the arch,

$$x=o \;\therefore\; r_o=\frac{n^2}{a} \quad . \quad . \quad . \quad . \quad (13)$$

Having found the radius of curvature, the next step will be to find the *direction* of that radius. This is done by the formula:

$$\tan.\theta=\frac{dy}{dx}=[\text{By (6)}]\frac{a}{2n}\left(\mathrm{E}^{\frac{x}{n}}-\mathrm{E}^{-\frac{x}{n}}\right) \quad . \quad . \quad . \quad (14)$$

At the crown where

$$x={}_0,\ \tan.\theta_o={}_0\ \therefore\ \theta_o={}_0$$

and the direction of the radius of curvature is vertical and coincides with ${}_0Y$.

We will now apply these formulæ to our bridge example. At the crown

$$r_o=\frac{n^2}{a}=\frac{(10.39)^2}{8}=13.5 \text{ feet.}$$

The direction of the radius at this point being vertical we lay off from H, in the direction HY, the distance HI=13.5 feet, when I will be the center of curvature of the curve at H.

We will now find the radius of curvature and direction for some intermediate points, E and F. The point selected should be where

$$x=\text{about } \tfrac{3}{5}x_1=(\text{in one case})\ \tfrac{3}{5}\times 20=12.$$

Putting this value of x into equations (12) and (14) we obtain: $r_{12}=25$ feet, and tan. $\theta_{12}=1.09725$ $\therefore$ $\theta_{12}=47°40'$. Therefore from the points E and F where $x=12$ and -12 we draw the lines EZ′ and FZ, making an angle of 47°40′ with the vertical, upon which we set off the distances EZ′=FZ=r_{12}=25 feet. These two points are the centers of curvature of the curve at the points E and F. Join and produce ZI and Z′I and we obtain the points F′ and E′, where the two sections of the curve are to be tangent to each other, that is, where the curvature changes. In the same manner we find the values of r and θ at the springing, where $x=x_1=20$, to be: $r_{20}=83$ feet, $\theta=68°40'$. Laying off from A and B the lines AK and BC, as before, and setting off upon them the distance $r_{20}=82$ feet, we obtain the centers for the curve at the springing, which centers joined with Z′ and Z respectively, and produced give the points E″ and F″.

Thus we see that the curve of the intrados from E′ to F′ is drawn about the center I with a radius of 13.5 feet. The sections E′E″ and F′F″, about the centers Z′ and Z with a radius of 25 feet, and the other two sections about their centers with a radius of 82 feet, there being five centers and three different radii.

In our example the intrados, drawn in this way, so nearly coincides with the original curve, represented by formula (6), that the difference is inappreciable in the drawing, and in all cases it will coincide sufficiently near for practical purposes. Therefore we now have an arch which has all the advantages of strength possessed by the catenary, while every portion of it is constructed about a center, with a known radius, giving it thereby all the advantages of the circular intrados, as each part can be as easily and readily calculated and cut as in the latter.

Let us now take a final case and go through the calculations throughout. We will suppose that we have an arch bridge to design with a span of 60 feet, a rise of 20 feet, and the distance from the crown of the arch to the road-bed to be 8 feet. It is to be understood that these dimensions can always be assumed or determined by the local conditions required, such as the amount of water or other way necessary in each particular case, and the formulæ given above will furnish every other dimension. Our voussoirs are to be composed of granite weighing 170 lbs. per cubic foot; the filling to be gravel weighing 110 lbs. per cubic foot. It is not necessary to be very particular about these weights, but it is very easy to ascertain about the average weight of the material to be used.

We have now all the data necessary and can proceed to calculate and make a drawing of one arch. The first step will be to find the value of n, which we do by equation (7). From our data we have: x_1 = half-span = 30 feet, $y_1 = 20 + 8 = 28$ ft., $a = 8$ feet. Introducing these values into (7) and reducing, we have:

$$n = \frac{30}{2.3 \log. 6.85} = 15.62$$

Referring now to Fig. 5, we draw the horizontal line XOX to represent the road-bed, the point O being directly over the crown of the arch. From this point draw the vertical line, OY, upon which lay off $OH = a = 8$ feet, and $OT = y_1 = 28$ feet. H is the crown of the arch. From T draw the horizontal line ATB, upon which lay off AT and BT equal $x_1 = 30$ feet. A and B are the points of springing of the arch. Our next step will be find the radii of curvature of the required arch at the crown, at the springing, and at one intermediate point. That intermediate point, according to our rule, is where $x = \frac{3}{5} x_1 = \frac{3}{5} \times 30 = 18$ feet. The corresponding value of y is found by equation (6′) to be 13.92 feet. Hence, lay off from O, OL and OK, each equal to 18 feet, from which draw the vertical lines LM and KS upon which lay off LF = KE = 13.92 feet. The points E and F are thus determined to be two points upon the soffit of the arch. We will now find the radius of the arch at the crown. We use for this purpose equation (13):

$$r_0 = \frac{n^2}{a} = \frac{(15.62)^2}{8} = 30.5 \text{ feet.}$$

Lay off, therefore, from H, $HI = r_0 = 30.5$ feet. I will be the center of curvature of the upper section of the arch. The next step will be to find the radius for the points E and F when $x = 18$ and -18. For this purpose we insert in equation (12) the values of a, n and x, when we obtain, by reduction:

$$r_{18} = \frac{[.066(10 + \frac{1}{10} - 2) + 1]\frac{8}{8}}{.0164(10^{.5} + 10^{-.5})} = \frac{1,91}{.06} = 33.5 \text{ feet.}$$

The direction of this radius is found by equation (14) to be:

$$\tan \theta_{18} = \frac{4}{15.62}(10^{.5} - 10^{-.5}) = .72984$$

$\therefore \theta_{18} = 36.° 7'$. Therefore, from the points E and F lay off the lines EZ′ and FZ making the angles SEZ′. and MFZ each equal to 36° 7′. Lay off upon these lines the distances EZ′ = FZ. = 33.5 feet, and we obtain the centers, Z′ and Z, of the middle sections of the arch.

In the same way we obtain the points Z″ and Z,‴ the centers of curvature of the arch at the springing, and by joining the points, Z″ and Z, and Z‴ and Z′ and producing these lines we obtain the points, F′ and E′. Now with I, as a center, and with a radius, HI = 30.5 feet we describe the arc, EHF. With Z and Z′ as centers, and with a radius, FZ = EZ′ = 33.5 feet, we describe the arcs FF′ and EE′. Also, with Z″ and Z‴ as centers, and with a radius, F′Z″ = E′Z‴ = 73.82 feet, we describe the arcs BF′ and AE′, which completes the soffit of the arch. The inclination of the arch at the springing, θ_{30} is 60°9′.

It will be noticed that in this particular case the curve corresponds very nearly with the arc of a circle having a radius of 32½ feet, the center of curvature being on the vertical line, OY. The greater the difference between the rise of the arch, h, and the half span, x', the more nearly will the curve obtained, by the above method, correspond to the arc of a circle. So that, in practice, when a bridge is to be designed, having a much greater half span than rise, after having found the proper curve of equilibriums by the above method, the bridge can often be actually constructed, without sensible error, about one center, C, the whole soffit, AHB, being the arc of one circle, the radius of which HC can be easily found in each particular case by the common formula : $R=\frac{h^2+x_1^2}{2h}$.

This, I say, can be done when it has been found by experiment, after having constructed the proper intrados, that such an arc of a circle of the radius, R, actually coincides so nearly with the former curve as to be practically identical with it. Of course, when the rise and half span are equal, this cannot be done as may be readily seen, from Fig. 4. In fact, it is to these latter bridges that this method is particularly adapted, as the semicircular and elliptical soffits are the ones which are particularly faulty in design.

Having determined the form of the soffit, we will now proceed to construct the curve, NPQ, which marks the joint between the masonry and the gravel filling between the face walls. This curve is determined by equation (10):

$$y''=\frac{p\alpha}{2}\left(E^{\frac{x}{n}}+E^{-\frac{x}{n}}\right).$$

In our example we have taken the depth of the keystone PH to be three feet, hence $p=\frac{OP}{OH}=\frac{5}{8}$. Therefore

$$y''=\frac{5}{2}\left(10^{.028x}+10^{-.028x}\right).$$

To find the points, N and Q, we introduce into this equation the value, $x=OV=OW=30$, when we obtain the corresponding value of $y''=VN=WQ=17.65$ feet. Any other point, such as U, may be obtained in the same manner by inserting in the above equation, the proper value of $x=OL$, and obtaining thereby the corresponding value of $y''=LU$. Thus the area included in the figure, VNPQWV, represents the space to be filled between the face walls, with gravel. It will be seen that it makes no difference how much heavier the masonry is than the filling, the soffit represented in the figure will still be the curve of equilibrium, provided the two are joined by such a curve as NPQ.

In fact, as many different kinds of material may be used as desired, if they are joined by curves represented by the equation : $y''=py$.

The horizontal thrust of the arch is given by the equation : $H=Wa^2$, w being the weight per cubic foot of the homogeneous material of an equivalent arch. That is : if W'=weight of granite=170 lbs., and W''=weight of gravel=110 lbs., we have :

$$aw=w'\times PH+w''\times OP$$

$$\therefore W=\frac{PH}{a}w'+\frac{OP}{a}w''$$

$$=\tfrac{3}{8}\times 170+\tfrac{5}{8}\times 110=132.5 \text{ lbs.}$$

Therefore: $H=132.5\times(15.62)^2=32328$ lbs. per foot of width of the arch.

It is not to be understood as necessary that the extrados of the masonry should conform in practice *exactly* to the curve, NPQ, but that curve is given as a general guide, to be conformed to as nearly as practicable, in order that the curve of pressures of the arch may pass as nearly as possible through the centers of the joints of the arch stones In fact, it might be well in most cases to fill the corners, NN' and QQ' with solid masonry, in order to more effectually resist the action of the traveling load.

In conclusion, it may be said, that the result of this investigation is as follows : Given the span, the rise, and the distance between the road bed and the crown of an arch bridge ; also, the materials of which the bridge is to be composed, the above formula will determine at once the strongest form for the soffit of the arch, and the proportions of the materials, to be conformed to in each particular case as nearly as the engineer shall consider practicable.

The nearer this conformation, the stronger the stronger the bridge will be, and, conversely the less the comformation, the weaker the bridge.

INDIAN SYSTEMS OF GEOGRAPHICAL MAP MAKING.

By Captain T. H. Holdich, R. E.

From "Royal Engineer Institute."

Of all the nations of the continent of Europe, two only have lately been engaged in geographical operations of a nature that might serve to give us valuable technical information in the particular branch of geodesy, which we call geographical surveying. France has been engaged in such work in Algeria, and Russia in Central Asia, and an examination of the systems of geographical surveying which they follow would doubtless be of very great interest—of great interest, but of little value, for there is this difficulty; in dealing with any foreign system as a source of information to guide us to sound conclusions, every system of mapping (like most systems in general) must be judged to be either good or bad by the excellence or otherwise of its results. But in dealing with French or Russian maps we have no guide whatever to their accuracy. We have obviously no power to apply a check or to sit in judgment on them, and consequently no power at all to say that their system of map making is either better or worse than our own. The most careful examination can lead to no definite conclusion, nor, speaking generally, can we say that what we know, and have known for years of foreign systems has in any material degree assisted to modify or elaborate our own system of survey. In fact, just as no nation has anything like the large interests that England has involved in this branch of science, so England also has the largest experience on which to base a scientific system of topography, and to our own surveys we naturally look to give us the guiding lines for its future extension. But this same difficulty of rightly estimating the value of maps applies to our own maps and our own systems. How are we to decide on the abstract value of any map? Having said of a map that it is accurate and highly artistic, it is quite open to any one else to say that it is inaccurate and most inartistic, and, indeed, this not infrequently happens even in scientific circles. What are the causes of fault in the matter? In the absence of public criticism, where is a fixed standard of excellence to which we may refer?

This is certainly a difficult question, but it seems to me that there are three guiding lines, three conditions or points of departure, as it were, from which we may proceed to adjudge the value of any map about which we can get full information. These three conditions are as follows:—What time was available for its construction? What did it cost? How far is it accurate? And relatively to these points we may either decide in the abstract that a map is good or bad, or we may bring any two maps together and say of one that it is better than another, and so, by means of the map, come to a similar conclusion in respect to the system under which it is produced.

And it may be pointed out that these three principal conditions serve a great deal further than mere points of reference to decide the value of a single map. They govern, one or other of them, every system of map making under every government in the world. If I say that accuracy is the guiding principle of our ordnance maps of England, I shall be sufficiently expressing the sentiment of every one here, without going into details to show the labor and time that is involved in their construction, or to prove that they cost per square mile 15 times as much as the ordnance sheets in India. It is enough to say that they are probably the most accurate maps in existence. Again, in all systems of work adapted to meet the requirements of field service, time fixes sharp limits of the work to be accomplished. To conduct survey operations in company with, or under cover of an advancing field force is one long struggle against time, as anyone must bitterly feel who has taken part in such operations. In India, again, the normal maps of the country are constructed as quickly as may be possible, and as accurately, consistent

with a certain fixed expenditure for the whole department. Economy is certainly the guiding principle of Indian surveys, and we consequently find in that land, where salaries are necessarily high and carriage always costly, that maps are yet turned out considerably cheaper than in any continental system.

In looking at any map, then, it is necessary to bear these three conditions in mind, or we may fall into the error of condemning a reconnaissance because it is not a survey, or an excellent geographical map because it will not tally with an ordnance sheet. Simple as they are, it is by no means unnecessary to insist on their recognition. Very unhappy has been the effect of a misapprehension of them even lately in India.

In looking to the future we must first decide on what are the probable conditions under which geographical maps will have to be made. Speaking generally it may, I think, be taken for granted that we shall have wide areas before us of rough uncivilized country, of which a knowledge of the leading features will be firstly valuable from a military point of view.

Time, which means rapidity of action, will be the guiding principle of these first surveys. Short and sharp are the military expeditions of the present day, involving hard marching and quick results. There will be no time to deliberate on the best system of work when that work has once begun. To get all the information one can in the readiest way one can, to turn to the best account small opportunities as they offer themselves, while retaining a general settled system of work which must be capable of much modification—a pliant system as it were—are the requirements of a modern military survey, to produce any definite results when the campaign is over. But looking a little farther into the future it is not difficult to foresee that beyond these first requirements there will be very large tracts of country, over which some more exact survey must pass than is sufficient to fulfill the purposes of a military reconnaissance, and which will be executed with none of the urgency of military movements attending it. Such a survey, in fact, as is now being carried over the native states of Hindustan, and which is extending itself even beyond our frontiers; such as has already been found wanting in Cyprus, and may possibly be very urgently wanted in Asia Minor; such as will most assuredly be required for the highlands of the Transvaal, and must extend itself with the tide of progress in our Australian colonies; and which even may, in a future which we may yet live to see, be spread in some modified form over those interesting countries which have only lately yielded up the great secret of the Nile.

But the urgent requirement of such a survey is usually a long way in advance of the means to pay for it. It frequently happens, indeed, that it is only through the agency of such a survey that the money-producing capabilities of a country can be justly estimated. It is frequently so in India, where the surveys of the native states are made at imperial expense. A useful, sound, geographical map, showing cultivated and cultivable areas, roads, rivers, villages, and valuable forest land, becomes a necessity to the civil administrator of a district, long before there is even a promise of recoupment of the expenses incident on making it. The guiding principle in the construction of such a map is, evidently, economy. It must be made as cheaply as possible, consistently with such a degree of accuracy as may insure its value in assisting the administration of government, or the assessment of revenue.

In considering the value of different systems under which such maps as these are produced, I think, for the present at least, we shall get the largest amount of information from our Indian surveys. Other countries besides England have had to carry geographical surveys through other lands than India, but the means are not forthcoming, as has already been stated, to enable us to estimate rightly the relative value of their maps, nor do such maps as are within our reach bear internal evidence of a higher degree of accuracy under similar conditions of time and cost than do our own Indian maps.

Moreover, the varied surface of India seems to present special advantages of study of almost all characteristics of topography that are likely to be met with in the great unmapped world.

India presents every variety of physical aspect, except that which is most familiar to us in England and Europe, of the most highly cultivated tracts supporting large manufacturing towns—and this, it is hardly necessary to observe, is just what geographical surveyors are never likely to deal with.

We may as well then, at once, begin with an examination of our Indian system of map making, and see wherein it differs from what is generally accepted as the English system, taught in our Military Academies and at Chatham.

Now the Indian system is so simple that, with hardly an exception (the exception will be noted afterwards) all classes of geographical surveying may be summarised in a very few words as—*plane tabling based on triangulation.* It may seem odd that such a well known old surveying instrument as the plane table should not, even yet, have arrived at the point of due appreciation. Used by every European country except England, and used by Englishmen most largely in India, it seems difficult to account for the fact that in England alone its scientific use is practically unknown. Of course, I am aware that it is occasionally used as an adjunct to a prismatic compass, or some other angle-observing instrument, but this is not what I mean by the scientific use of it, and I think I am justified in saying that its capabilities as a scientific instrument are practically unknown. It is due to the memory of one of India's most able Engineers (Colonel Dan Robinson, who lately died at the head of the Telegraphic Department of India) to say that it was his foresight that first pointed out the use of the plane table for Indian surveying. In it he found an instrument so simple that any intelligent native could readily be taught to use it, an angle-observing instrument far exceeding in accuracy the prismatic compass, and a traversing instrument of the greatest value, from the fact of its being totally independent of compass error induced by local magnetic attraction. It has been in use now for many years, during which many officers have learned its capabilities while employed in the survey of our great Indian dependency. As each one, in turn, has been introduced to the system, he has admitted its value to be greater the more thoroughly he has become acquainted with it, so that of every officer in the Indian Survey Department, I think it may now be said, that, were he asked how he would propose to make a map of any rough piece of country before him—from a geographical survey to a rough military reconnaissance, he would unhesitatingly reply, "Let us plane table it." There is not one, no matter how he may have been originally taught, who would adopt any other method.

Now any system which inspires such general confidence as this, is surely worth attention at present, and a fair trial in those large and important fields of work which yet lie before us.

The normal system of Indian surveying is as follows—the system being only modified to meet the necessary requirements of time in the case of military expeditions: the ground is first triangulated from an efficient base, which base in India is invariably furnished by a side of one of the great trigonometrical triangles, which are extended in longitudinal and meridional series over the face of the country. Even in the case of our military frontier expeditions such a base is generally procurable—but we must deal with such surveys as forming a distinct class. The triangulator who uses a 10″ or 12″ theodolite will usually take a plane table with him, and carry on a geographical reconnaissance hand-in-hand with his angular observations. If time admits, he passes over the ground first with the plane table, previous to any instrumental triangulation whatever, as it is in this way that he can best assign, first, the proper position and proportion of points which are to form trigonometrical stations, and next, the subsidiary or secondary points which are fixed by three or more intersections; and can detail the necessary working parties for clearing jungle and erecting signals. Now, though this is merely a rough triangulation chart, its use, when the instrumental work begins, in pointing out the approximate position of signal stations, and in defining the field in which to search for them, when in a background of forest and hill they would often escape detection even with the most powerful instrument, is enormous; and the surveyor will often be surprised

to find when the final computations are elaborated, and he is able for the first time to plot accurately by latitude and longitude the position of his trigonometrical stations on his plane table, how accurate this reconnissance, carried out with no aid of compass or protractor (both of which introduce their own class of error) proves to be.

But it not infrequently happens, as I have said, that from the excessive wildness of the country and difficulty of moving about in it, it is most unadvisable to pass over any of the ground twice. The erection of many signals becomes an impossibility, and the selection of trigonometrical stations even a matter of great difficulty. Under pressure of time it becomes necessary to triangulate without any previous reconnaissance whatever. In a country presenting no other features than forest clad peaks, rising with painful similarity of feature and monotony of color from dull forest clad plains below, where the highest tree that may show its top somewhat above the level of those around it becomes the only available signal from day to day through trackless miles of interminable jungle, it would be totally impossible without a plane table chart of this description to unravel the recorded observations to hill peak after hill peak, and finally place each in its proper position. And we must expect much of such work as this in the future. Into the details of triangulation and the nature of signals it is impossible for me to enter. I will merely add that true economy of time and labor consists in carrying a triangulation, once commenced, right through to its completion—never passing twice over the same ground, never revisiting a station once occupied. To attempt first a bit of triangulation, and then a bit of topography, or to carry on the two together, hand in hand as it were, over the same ground, will never lead to a large out-turn of work. The reasons are obvious, but the effect is best noted by the fact that though it would be possible by making steady and direct progress through even the worst country to triangulate, say 4,000 miles in 10 weeks to three months, yet it would never be practicable to do 400 in the first week of the 10. I may further state that all such triangulation being for the subsequent use of plane tablers, we find that one trigonometrical point for each 70 to 100 miles—and one secondary for each 10 square miles is ample for 1-inch work, or about half of what would generally be considered sufficient for topography by any other system of fixings by interpolation.

Neither in India, nor elsewhere in country similar to India, is it necessary (in fact it is not possible, consistently with our guiding principle of economy) to employ only highly skilled labor for the purposes of topography. It is true that a proportion (varying in different districts with the peculiar difficulties presented in the course of survey) of every survey party must consist of skilled draughtsmen, who have sufficient mathematical capabilities to enable them to compute ordinary triangulation, during the season when the climate interferes with the field work, and who complete the final mapping by making fair copies of the field sheets; but if the superintending officer is prepared to compute his own observations, his draughtsmen might be draughtsmen and nothing more. Given that a man has a capability for drawing (a capability which most natives of India possess more or less) he is at once suited with an instrument in his plane table, which enables him to take his place as a surveyor at the minimum cost of expense in teaching. He need know nothing about angular measurement; he need not be able even to read a compass; he never has an observation of any sort to record. The system of fixing his own position by interpolation by the simple process of looking through the sights of his ruler, is one which commends itself to the most ignorant mind for its very simplicity. If three rays intersect he knows he is right (there is just a possible exception to this), if they do not he is wrong, and all he has to learn is which way his table must move in azimuth to correct the azithmuth error and bring the rays together to a point. It is usually the practice to make use of the compass to get the approximate azimuth in the first instance, but the true azimuth is determined, not by it, but by the due intersection of rays. And it must be remembered that this intersection can be made as minutely ac-

curate as it is possible for a pencil point to define it.

Those accustomed to the system of mapping from points fixed by interpolation from prismatic compass observations will remember that there are four distinct origins of error in the system. Firstly, the compass error induced by local attraction. With this error, so far as I know, it is impossible to deal, although it must have fallen within the experience of every geographical surveyor to have found himself often in positions when his compass was absolutely useless. I confess I do not know how this difficulty has been overcome, but it would, in some parts of the world that I have been in, prevent map-making on this system altogether. Secondly, there is the error of compass observation. To what degree of accuracy can an observer take an angle with a prismatic compass? I think 20″ is probably near the mark. Thirdly, there is the error of a graduated protractor. Assuming that a circular machine-made brass protractor is used, the average error (I have tested many) is about 15′. Ivory protractors are so absolutely useless from this source of error that I presume nobody uses them. Fourthly, there is the error arising from inaccurate protraction of the observed ray, an error (whatever may be its value) that is proportionate to the length of the ray or line protracted. When this line is finally laid down in the sketch sheet, within what limits can the surveyor guarantee its final accuracy? Shall I say half a degree? I think a degree would be much nearer the truth. And if three such protracted rays do not intersect, to which error of the four must the divergence be assigned, and what is to determine the relative value of these rays? It is clear that fixed points from which to interpolate must be close to the observer, and there must be many of them, from the tendency to increase of error with the length of the ray. And if we fix a limit of distance beyond which no point should be used, the triangulator must remember that the number of points he has to lay down increases in an inverse ratio with the *square* of that distance. Now an average plane table for 1-inch survey work contains about 450 square miles, or four geographical reconnaissance on the ¼-inch (a very useful scale for this sort of work) 450×16, or 7200 square miles of country. And if in all this vast area there be four fairly well placed, easily recognized, fixed points previously laid down, which points need not by any means necessarily be visible from every point of that area—but only from positions of important elevation within it—there is quite sufficient means for the topographer to commence his map at once. The four origins of error are reduced to one. There is no compass error, as there need be no compass. There is no protractor either, but there is one source of error in the angular accuracy of the actual observation through the ruler. The value of the plane table as an angle observer has been variously estimated at from 5′ to 10′. I am inclined to put it at 5′. 10′ subtend 15 feet at a mile, and a 15-foot signal would afford a very definite center at that distance. Suppose the azimuthal adjustment to be slightly inaccurate, the intersection of rays from short distances would still be good, and the error unobservable; but as this error increases with the length of the ray, intersections from long distances would gradually become worse till the rays ceased to meet in a point. But the nature of the figure enclosed by them when the intersection fails, at once reveals the extent of the error, which can have but one assignable cause, and the correction is easy. This is why far distant points are used as references for adjustment in azimuth, while it is found advisable to have, if possible, a fair number of points scattered over the board to prevent the loss of time occasioned by the necessity of very fine adjustment. The accuracy of the plane table has been defined as limited only by the fineness of the point of a hard pencil. Compared to the prismatic compass as an angle observer, I should be inclined to reckon it in the ratio of 5′ to 40′ or 50′. Nor must the advantage of working on a steady immovable table, which is truly parallel to (that is to say in true azimuthal relation with) the country to be surveyed, be overlooked. It saves a great deal of troublesome thought and care; while the saving of *time* effected may be easily reckoned up by anyone who will compare the processes of simply observing and drawing a line on a level table, with that of observing with an un-

steady compass, and then protracting, first, the angle and then the ray, in a cramped position on an equally unsteady sketch sheet.

Natives of all classes show special facility in acquiring a knowledge of plane tabling, so that a very large share of the topography of India is now laid down by them, and for the future we must look largely to all such local agency in carrying out geographical operations at anything like a reasonable cost.

The net result of the system is this: Admirable 1-inch maps are turned out, often of the highest quality in point of accuracy that is attainable in maps published on the same scale as the field sheets—and always of a good average in this respect—at $\frac{1}{15}$th the cost of the 1-inch ordnance maps of England, or about £2 per square mile. But it might fairly be doubted whether the plane table is suited to all classes of ground alike that are to be found in India, or that might be found in any country possessing about the same wealth of cultivated land. India offers us a large variety. There are large expanses of wide, sandy desert, with details of topography but sparsely scattered through them, and many of the natural features shifting from year to year under the influence of climate. There is the normal condition of hill and plain, with a certain proportion of fine natural landmarks that have to be supplemented by auxiliary signals. There is much of flat and well cultivated plain, difficult to survey from its exceeding flatness, and the preponderance of large trees in clumps, or *topes*, and lastly, there is *very* much of wild jungle-covered country, nothing but rank growth of forest trees and forest grass, where one may ride for many a day's journey without finding a natural landmark of importance, and where every artificial signal has to be hunted for. How are we to deal with country such as this without reducing the value of the survey to that of a mere reconnaissance, or without increasing the cost to excess by clearing lines of traverse and points for interpolated fixings. The experience of the last few years has taught me that this is a very difficult problem, but that it is best solved, after all, by the use of the plane table.

I need not explain in detail all the different systems of traverse, but that is, perhaps, best by which the traverse is laid down on a large scale, on a sheet of paper pinned down over the board, and projected (after reduction to the proper scale) at intervals into the map, whenever any opportunity for check by direct observation to any station or signal on its flank may occur. Traverses may be made to converge to a point, or to "gridiron," and so check one another. Every check that can be gained is brought into play at once, and its effect noted on the ground. Errors of chain measurement are generally detected almost as soon as they occur, and on the whole this system of traverse work may be said to be very fairly satisfactory. So that in all circumstances, and in every class of ground, plain tabling may be classed as the very backbone of Indian surveying.

But having so far described what is done in India, it would be most instructive to examine what has been done by English surveyors elsewhere, under similar conditions of time, cost, and accuracy; such as may guide us to a relative appreciation of the respective results. A certain amount of topographical work was executed in connection with the North American Boundary Commission, but its execution, in the matter both of time and cost, was so entirely subsidiary to the boundary definition, that it seems impossible to separate the two classes of survey. As it was, moreover, but a narrow strip of country, extending nowhere more than a few miles from the boundary which furnished its basis, it does not present an example of so much interest as others that we can find.

The survey of Palestine, which has been conducted during several years by Royal Engineer officers, and worked by Royal Engineer draughtmen, is, under all its conditions, more nearly similar to the geographical work in India, and gives us a more instructive example, because, though of small extent, it has been large enough to test fairly a definite system. With regard to any remarks I may make on this survey, it must be understood that I am indebted entirely to the kindness of Lieut. Kitchener for any information which I possess, and that such remarks refer only to that part of the work which has been conducted by him. But

Lieut. Kitchener's party included some fairly experienced hands, who might be supposed (as I feel sure was the case) to realize the best possible out-turn. The scale of the work is just that of the normal Indian Survey. The object aimed at was finally the same, though there was probably much work, extraneous to the simple surveying, in archæological and other scientific examinationss of sites, and in various reports, for which due allowance must be made. The ground was easy to survey—as I believe is the case throughout Syria. I should have thought it was very easy, but for the assurance to the contrary of those who mapped it, and who ought to know best. But here again we come to the necessity for a definition, and I can only appeal to the decision of those here who may happen to have worked in India, as to the nature of the ground. It is generally open, with here and there strips of sandy plain, almost amounting to desert in their characteristics. There are no forests of any great extent, nothing of the nature of what we generically term "jungle." The face of the country consists partly of hills of tolerably abrupt conformation, and partly of open plain, here and there much cut up with ravines. There are such numbers of natural landmarks that the triangulator laid down about six or seven times as many points as would be furnished in a similar area in India, without ever clearing a ray or erecting an artificial signal. It follows of course, that nowhere could the topographers well put themselves out of sight of three or more such points. Finally, I think I am correct in saying that the topographers could ride to their work over pretty nearly the whole area. The difficulty possibly lay in the amount of detail which (although the final maps show no excess) may have led to confusion in the selection of the most important features. On the other hand, in Sir Rutherford Alcock's address to the Royal Geographical Society last May, we find, with reference to Indian Surveys and to the work of Lieu. Harman, R. E., in particular, "The work was very difficult, incessant rain for many days not only flooded the nullahs and turned the forest paths into streams of mud and water, but brought out myriads of leeches, while cane jungles formed almost insuperable obstacles to laden elephants. Lieu. Harman found magnificient specimens of rubber trees, and in one of them a trigonometrical station was formed 112 feet above the ground to connect his triangulation with that of Lieut. Woodthorpe, R. E. Lieut. Harman was laid up, &c."; and of Lieut. Woodthorpe's work he says "In one place a range of hills is described as nearly level along the top, with no commanding point anywhere, it is sinuous and covered with tall forest trees filled in with a tangled undergrowth of bamboos and canes, through which we cut at the rate of 300 yards an hour." And again "The survey of the river was difficult." In some places "it took three horses to make a quarter of a mile of way," and so on. There is plenty of such work in India; but we see, at any rate, that there must be wide distinctions between different classes of country, and I think we may fairly call Palestine comparatively easy. As to the time occupied and the cost of the field sheets (not of the *final maps* please observe) of the survey, it appears that about 1000 square miles were surveyed between the end of February and the 10th July. This I make out to be about 18½ weeks, and would give an average of about 18 square miles per topographer (for three men) per week, if we leave the topography done by Lieut. Kitchener himself out of account. He could, at any rate, have only devoted a part of his time to the mapping, and we shall get a fairer average, perhaps, by leaving it out of account. The estimated cost was about £900 for that amount of field work, or about 18s. per mile. So far as one can judge from what must at best be only an approximate estimate, topographers of about the same technical skill as draughtsmen would turn out a considerably larger area in similar ground in India. I must speak from my own experience only, if I say that 25 to 30 miles would be expected of them. As to cost, if we estimate the cost of the field sheets only (which is what we have at present to deal with) I think we should find the cost of the Indian Survey somewhat greater, say from £1 to 25s. But it must be borne in mind that the salaries of Indian Surveyors range from £60 to £600 per annum, and the salaries of the superintendent from £600 to £2,000, or more; while the con-

ditions of cost must otherwise be so widely different in the two cases that we can institute no fair comparison, and must appeal finally to the test of accuracy. This is a difficult matter to deal with, and it might be fairly said that only a comparison of the field sheets of the respective surveys by competent critics could decide anything, and much do I regret the impossibility of procuring the field sheets at present. I must emphasize the fact of the field sheets affording the only safe and practicable guide in the matter, and emphatically assert that the final maps as reproduced by photo-zincography afford no criterion whatever. But still there is a test of the value of such maps, one constantly applied in India, and held to be, in the main, satisfactory, and this is the record of the number of interpolated fixings of his position made by the topographer in each square mile. I am very far from saying that this is a perfectly unerring guide, nor do I quite agree with a high authority (perhaps the highest) in India, who unquestionably condemned maps of a hill district, of which he admitted the high artistic merit and apparent consistency of detail, because there were only one or two such fixings per square mile; I can conceive that in that case those one or two were sufficient, but still this is, in the main, a satisfactory test, quite applicable in the case before us. This average in the Palestine survey, I was told, (but I think that careful examination might alter the figures) was about two per mile, which would, on such ground, be considered hardly sufficient in India to complete a fairly accurate reconnaissance. This is just what I understand the Palestine maps claim to be—a reconnaissance of the ground between the Jordan and the Mediterranean. But we most distinctly claim for our Indian 1-inch maps that they are not reconnaissances at all, but surveys, and we should require an average of at least six or seven (rising perhaps to ten) fixings per square mile, for a country such as that, to be as accurately sketched in detail as the scale will admit. And this conclusion is just what I should expect. *Ceteris paribus*, a man using the plane table would certainly be able to fix his position twice as often and sketch twice as much—with far more accuracy—as the man who has to observe each angle with an unsteady compass and protract it with an inaccurate protractor on a shifting piece of tracing paper. Yet another point in connection with the Palestine surveys may be stated, as furnishing some indications of the general style of survey turned out by the use of prismatic compass. Whole villages (without any definite point in them) were found to answer the purpose of signals, or fixed points for observation. I can quite imagine this to be so—but it would not do to offer such a point to a plane table workman—from what has been said it should be clear, I think, that the difference between the two systems, as illustrated by such results as we have been able to get at and compare, amounts to this—the plane tabler will effect a survey where the prismatic compass observer will produce a reconnaissance, and I think such a conclusion represents the situation pretty accurately. It may be said that the reconnaissance is all that is wanted—that it is quite good enough. No map is good enough that could have been much better for the same cost, or that might have been done on half the scale in a quarter of the time with the same amount of accuracy.

We will next consider our Indian system of map-making as applied to the work of a reconnaissance—a field map executed in conjunction with an army in the field under stringent conditions of time. I need not enter into details of the circumstances under which such maps are usually made, but it should be remembered that the time available for such work is usually but a small part of that occupied by the campaign or expedition, as the work of the surveyors must almost always be abandoned on the backward march of the troops from the furthest point gained by the advance, as it is only under cover of an advancing force that commanding points for observation can be occupied, and it seldom happens that surveyors can be allowed to remain behind, or even far from the main column. Time, again, is limited by accidents innumerable, which are certain to arise to cause delay and bar the progress of the work. The importance of being early in the field is very great. The surveyors should be on the ground as soon as they can obtain a footing, as there are casual opportunities of gaining valuable

information at the commencement of the campaign, which may never occur again. At present the information gained by the preliminary reconnaissances of the officers of the Quarter-Master General's Department add little or nothing to our geographical knowledge, but there is no reason why it should be so, were those officers invariably acquainted with the use of the plane table.

It is, indeed, most especially in this branch of the surveyor's art (that of military reconnaissance) that the value of the plane table is most strikingly illustrated. Indeed, we may say, that it is only the introduction of the plane table system that has, at last, put into our hands the means of acquiring that widespread and thorough military geographical knowledge which modern science demands. Never again can there be an excuse for turning our backs on a country without such a complete and thorough knowledge of it at our disposal, as shall definitely guide the course of all future campaigns. It is no small thing to reduce to a scientific map the grand chaos of mountain and valley, that bewilders the eye and depresses one with the sense of endless confusion, at the first glance over the mighty northern mountain chains of India, when the only basis for the map are some four or five widely scattered snow peaks whose cold sides defy all human approach. And it is no small system that will help us to do it. The prismatic compass cannot help us here. We must have a broad sheet before us representing at one view many thousands of square miles, or we shall be unable to make one of the few points which are all we can get. We must have the power of minute accuracy to enable us to reduce the scale sufficiently to get those thousands of miles into a portable board. We must have perfect steadiness and no variable compass on those iron hills.

The system which has stood the test of severe trials in India, and promises best towards further developing this branch of science, is but a modification of what has already been described—"plane tabling, based on triangulation."

All along the western and northern frontier of India from Afghanistan to Bhootan, a number of out-lying points have been from time to time laid down by triangulation from within the frontier, comprising peaks of the great Himalayan chains, conspicuous by reason of their height or form, so that it is only necessary for a surveyor to determine what must be the scale of his map, so that, on a plane table of the largest dimensions compatible with portability, he may introduce five or six of such widely scattered points within the limits of his board, for him to have, at once, a practicable, if not always very adequate basis for topography. Bare measurements and preliminary triangulation thus disappear, and a great advantage is gained by work, on a plan which embraces a large area of country, which advantage increases with the number of points thus secured within its limits. This is no new system. Admirable maps have thus been made by Colonel Godwin Austen, of the Bengal Staff Corps, and Captain C. Strahan, R. E., and latter by Lieutenants Leach and Woodthorpe, Harman and Major Badgley. Such work is constantly in progress, and in this way we are gradually extending our geographical knowledge beyond our Indian frontier, and shall, doubtless, eventually have a perfect acquaintance with that great debateable land which lies between us and the Russian frontier. The points of peaks so fixed serve also as points of reference to another class of geographical surveyors altogether. These are the plucky native workmen who under various disguises penetrate into the dreary steppes of Thibet, and bring back at the risk of their lives geographical records of countries absolutely closed to Europeans. This is, indeed, geographical map-making of another and most interesting type, which can hardly as yet be classed as reconnaissance; its high importance has been most fully recognized by the Royal Geographical Society by the award of its medals to Colonel Montgomerie, R. E., Captain Trotter, R. E., and last but not least to the gallant old pundit Nain Sing, native school-master in the district of Kumaon, who may yet live long enough to stir up a rising generation to similar feats of pluck and endurance.

But it does not always happen that we can start with the advantage of points trigonometrically fixed as a basis for map-making of this sort. There are other countries than India equally wor-

thy of the attention of geographers, which have, as yet, no absolutely fixed value of latitude and longitude, whose places on the world's surface may still be called indefinite. Such was the nature of things in Ashanti. I am not aware, though, what (if any) attempt was made to lay down a scientific basis for future geographers in that by no means unimportant corner of geographical *terra incognita*. It does not appear in Colonel Home's report. But the effort was made in Abyssinia, with results too that, in view of the new relations springing up between England and Egypt, and between Egypt and Abyssinia, are growing in importance every day. The line, nearly 400 miles in length, then accurately defined along the main water-parting of North Africa, between the Mediterranean and the Red Sea drainage, served as a base from which peaks were fixed along its flanks, which might even now be sighted from the furthest advanced points in the reconnaissance of the head waters of the Nile, laid down by officers under Gordon Pacha's command. The plane tabler could doubtles bring them together and bridge over a tract of most interesting country, which, at present, divides Egypt from Abyssinia, and that in a land where knowledge of the country means simply security of possession. There is no geographical knowledge in that portion of the globe that will not prove, at no very distant date, of the highest political, if not commercial value. The operations in Abyssinia had to commence with the measurement of a base; observations for absolute latitude, longitude and azimuth were taken at either end, with all the accuracy that the use of first class instruments would admit, the longitude being determined by observations similar to those used in the definition of the North American Boundary. Thus the whole of the work has an absolute value of its own on the earth's surface, which must be accepted as a satisfactory reference for that part of the continent, until observations of a still more rigidly accurate nature can be taken elsewhere in North Africa.

From this base some preliminary points were laid down by triangulation, which gave the basis for topography from the sea coast to Senafé on the high land, where another base and further triangulation carried it on to Antalo. The ends of the bases were connected by an instrumental traverse, which was run right through the entire route from end to end. A treble value by observations for latitude (the route being nearly north and south gave these a peculiar value) and longitude by traverse, and by plane table were thus secured, and it was found that the error by the plane table (on the $\frac{1}{4}$-inch scale) was so small as to be unapparent. A fresh base at Antalo and another at Magdala completed the triangulation necessary, but beyond Antalo the topography became thin and weak. The party was worn out, and there was not an officer with the force capable of using a plane table (though there were many whose services would have been available) and other methods failed miserably when brought to the test of actual practice, at the pace which it was necessary to maintain. In all, however, above 5,000 square miles of actual mapping, and about 400 miles of a traversed route, along the most important line that could have been selected with a view to the future extension of geographical work in North Africa, including the verification of much doubtful information supplied by previous travelers, was secured by three officers and one non-commissioned officer of the Royal Engineers, from the effective strength of which small party a large deduction must be made for sickness. It was a striking illustration of the value of the plane table, and we learnt from this expedition:—1st, that the route traverse might have been dispensed with, as only affording an additional check, and supplying no geographical information whatever; and, 2ndly, that all this, and very much more than this, might have been easily accomplished without the expense of any special survey party whatever, had the officers of the Quarter-Master General's Department been well instructed in the use of the plane table. I have avoided touching on the question of the value of the geographical results thus obtained, either from the geographical or military point of view, because the value has been fully discussed before. Geographers will at once agree that all new information, whensoever and whereso-

ever obtained, has its value; and the effect of the various topographical features of what has been called the Earth's Crust on the conduct of a campaign, has already been treated with the greatest clearness in Hamley's *Operations of War*, and other standard military works. It is the best method by which to obtain a knowledge of such features with which we have at present to do.

I have also left untouched the question of the adaptation of the plane table to the work of ordinary military reconnaissance, beyond stating that any military officer of the Indian Survey Department would most certainly use the plane table for such work. But it is not the sort of work which has fallen much to the Department, and I should be wandering outside the realms of hard experience and fact into those of suggestion and theory, which I have no wish to do. *Rapidity and accuracy* appear to me to be as fully important in this branch of military survey as in all others, and the plane table would lose nothing by the additional capability of a contouring or leveling instrument for work on a larger scale. Possibly an objection might be raised on the score of portability. We are not in the habit of bestowing much consideration on this point in India. Our 24″ theodolites find their way up the steep sides of almost inaccessible peaks, and we have come to think very little about a pound or so more or less in the weight of an instrument; consequently our Indian plane tables are not very portable. But a plane table is a drawing board on three legs, and it would be an insult to mechanical ability to suggest that it cannot be made just as portable as you please. Messrs. Troughton & Simms have just made one for me, which I could easily carry myself and, on the same construction, I have the satisfaction to find that it might have been made half the weight without in the least detracting from its value.

It is another class of map-making to which we must refer next. There may be weighty reasons (physical or political) which preclude the use of all instruments of the size and nature of a plane table. Where, for instance, observations have to be taken at the risk of life, and progress can only be maintained in secret and under disguise; here we are dependent on compass observations, on routes measured by pacing, on astronomical checks such as can be obtained with a small sextant. Even the pace of an advancing force (as in the case of the Russian advance on Khiva, in which the expedition under General Vereokin marched from Orenburg to Khiva, over 1,000 miles, between the end of February and the beginning of Juné, reaching Khiva at the same time as Kauffman's expedition from Tashkent) may be too great to admit of much more than this. But this sort of map-making is of, perhaps, the highest importance of all, both from its being within the power of every traveler to accomplish, and from its having lately developed to a remarkable extent by the employment of trained natives for Trans-Himalayan explorations. I think that the experience gained by work done in India within the last ten years points to at least one consideration, which, if well weighed by travelers who aspire to the acquisition of a really scientific knowledge of the geography of the country they explore, might lead to valuable results. This consideration is the readiness with which all classes of natives, whose instincts have been sharpened and habits formed by the constant influences of nature herself, and familiarity with her secrets, seize on the main principles of map-making, and become, with a little pains in teaching, valuable aids to the acquisition of geographical knowledge. The general meagerness of the information supplied by most travelers is doubtless due to an over anxiety to be able to see and attest all geographical facts with their own eyes, added to the temptation of thrilling personal adventure, to leave alone the slow process of compiling the results of observations of others. No one can wonder at this. Still there appear to be points in the progress of all great explorations at which the resources of the white man are at an end, at any rate for a time. But what the white man cannot do, the native frequently can. He is apter at disguising himself, can support himself by ways and means which we know not, and, if at all accustomed to travel, can measure his paces through the long weary day with a persistency and accuracy that sometimes seems marvelous.

Two men, started by different routes to the same point, and bringing back each his tale of paces, will lay down approximately the position of any such point, and if we add to this a slight familiarity with the use of the simplest instruments, such work can be done as you may read of in the reports of the Trans-Himalayan explorers during the last ten years.

Looking at the Transvaal with reference to the 600 or 700 miles of undefined country which lie between its frontier and the head waters of the Nile—or at the most advanced posts on the Nile near Gondokoro, with reference to the debateable land between them and Abyssinia; or the marvellous lake region of which we hear so much, who can doubt that the man who first resigns the hopes and delights of personal adventure for the unpleasant process of shaping out a map from the observations of others, instructed and trained at that frontier, or at those advanced posts will reap a rich reward of geographical knowledge. Another suggestion might possibly be of value. If every traveler, who keeps a chart of his travels, would mount that chart on a plane table, he would preserve it better, and would certainly at once double his capacity for adding to the topographical records of his map within any given limits of time. As he became more conversant with the use of the plane table, he would more surely find the thin red line across a blank sheet of white paper, which usually shows a traveler's footsteps, expand itself into something like a sound illustration of the topographical features of the country generally.

Briefly what I have endeavored to show is as follows:

1st. Systems of map-making must be judged by their results, of which the relative merit may be gauged by due consideration of the conditions as to time, economy and accuracy under which they are obtained, and, as far as we can judge, the best results have been obtained under the Indian system.

2d. That in all the vast field of mapping which yet lies before us, we are likely to arrive at the best results in the shortest time, and at the least expense, by a careful application of the main principles which have guided us to results in the survey of wild lands similar to those we are likely yet to find.

3d. If we accept this as being, even possibly, true, then it follows that the use of the plane table should become general. It should not merely be an instrument in the hands of a few scientific men, but every officer who may ever be possibly called on to make a reconnaissance, or lay out a route, should be thoroughly master of it. So far as the wide area of India, and its nothern outlying states are concerned, the system is established, and it merely becomes a question of whether a man shall learn his work when he comes to do it for the first time, or have a previous knowledge of it, such as he would have of road or railway making, or barrack building. So far as this is concerned, it may be a matter of no vital importance, but there are times (is there not such an occasion at present?) when the knowledge of the system possessed by a few Engineers at our head-quarters, or a few officers of the Quarter-Master General's Department, would be of the utmost value in the pressure of a campaign, when the burden of this, the most trying and severe work that a military man can engage in, falls upon the back of the over-strained Survey Department of India, when it may be that knowledge of the first importance must escape our grasp because there is no one to reap it. But there are other wide unmapped lands before us. Geographical discovery is the heritage of this age, and close in its footsteps follows geographical mapping. Would it not be well in England to make an honest trial of a well-established system?—a system that has proved its strength—a system finally that has the unhesitating support of every single scientific man who has tried it.

The weight and brittleness of terra-cotta are great objections to its use in interior architectural decorations and mouldings, and for household utensils and ornaments. To avoid these objections a Spanish South American firm employs cotton pulp covered with a special composition, which contains a soluble varnish. Articles which are made with this material are very light and strong.

ON THE SHAPE AND SIZE OF THE EARTH.*

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I. The Earth as a Sphere.

When surveying is carried on with such accuracy, or over so great areas, that it becomes necessary to take into account the curvature of the earth it is called Geodesy. Were the surface of the earth a plane, as certain ancient peoples supposed, the science of geodesy could never have arisen, since measurements founded on the geometry of Euclid would be capable of determining accurately its geographical features. In fact, however, such measurements become more or less entangled in discrepancies according to the size of the country over which they are carried. For instance, beginning at a certain point, let a line be run due east for half a mile. At any point upon it, its direction, found by common methods, would be the same as at the starting point; but let the line be produced and it will be observed that its direction is deviating from east, and that this deviation becomes more and more marked the farther it be prolonged. If the starting point be at New York, such a line would, in fact, deviate to the south until it crosses the equator in Africa and passes through or near Australia. Again, let three points be taken on the earth's surface at considerable distances apart; the sum of the three angles thus formed, will be found, if measured by an instrument whose graduated arc is placed level at each station, to be greater than 180°. Or, to use an illustration from American surveying: let us consider the system for the division of our public lands, the law concerning which provides that they shall be laid out into townships "six miles square," with sides running duly north and south or east and west. These two requirements, perfectly possible were the earth a plane, are in practice impossible, and the areas of the townships are only laid out "as nearly as may be" to the legal required quantity. From these and many other discrepancies we conclude that the earth's surface is not a plane.

Reasons for supposing the figure of the earth to be globular are given in all the text books on astronomy. They are: the appearance of the top of a light-house before its base to a ship approaching port, the dip of the sea horizon, the elevation of the pole star as we travel north, its depression as we travel back and the new stars that come to view in the south, the analogy of the other planets which seen through a glass seem to be globular, and lastly, the circular form of the earth's shadow as seen in a lunar eclipse. To these must be added the well-known fact that travelers, going ever eastward, pass entirely round the earth, and return again to the point of starting. We regard it then as proved that the earth is globular; that is to say, like a globe, but whether spherical, or spheroidal, or ellipsoidal, or ovaloidal there is thus far no evidence.

The importance of determining the figure of the earth will be apparent when you reflect that map projections of all kinds, and hence the accurate representation of the geographical features of its surface, depend upon it. Particularly to the mariner on the sea is such knowledge valuable, for upon his values of the lengths of degrees of latitude and longitude depends the accuracy of his calculations and his safety. From an engineering point of view we have, indeed, to enquire into the advisable precision to which it is necessary to carry the determination, and this will be alluded to in a following lecture. From a scientific point of view, however, the investigation is not limited by considerations, either of necessity or economy, but is merely one branch of the problem which all science is endeavoring to solve; namely, how and why did this earth and its inhabitants come to exist. In our treatment of the subject we shall look at it mainly in an engineering light, although the scientific aim will not be at all forgotten.

To obtain exact information regarding the figure of the earth, precise measurements on its surface are necessary. The most natural method of procedure is to assume the form to be spherical, and to

* Three lectures, originally prepared for the Civil Engineering Students of Lehigh University as introductory to a course in Geodesy.

test the hypothesis by observations; then, if this be found not satisfactory, to assume it spheroidal, and to make further measurements and calculations. This is the plan which has been followed by scientists, and it is difficult indeed to conceive of one more feasible, since here, as in all science, each step in advance must be from the simpler to the more complex, and be suggested by the knowledge already attained. To assume the form spheroidal at first would be more or less impracticable too, for exact calculations regarding a spheroidal triangle, for instance, imply a knowledge of the eccentricity of the meridian ellipse, the very thing required to be found. In this lecture, then, we regard the earth as a sphere, and proceed to discuss the methods by which its size may be determined.

And first of all we must decide what is the surface whose form is to be investigated. This can be no other than that of the waters of the earth. The ocean covers the greater portion of the globe, its surface is regular compared to that of the land, and although it is agitated by winds and raised in tides, the position of its mean surface is capable of being located very accurately. Moreover, the land is really elevated but little above the sea when compared with the great radius of the globe. The mean surface of the ocean is, then, the spherical surface whose radius is to be determined.

An approximate value for the radius of the globe may be found by observations made at sea upon the distance of the visible horizon. It has been noted, for example, that two points distant about eight miles apart are just visible one to another when each is elevated ten feet above the sea level. Let a plane be passed through these two points, cutting from the globe a circle, and from the center let lines be drawn to the point of tangency and to one of the points of sight, forming a right-angled triangle, from which, with the given data, it is easy to find

$$r = \text{about } 4200 \text{ miles}$$

for the radius of the globe. This value, as every one knows, is in excess by 200 miles or more, yet reflection upon the rude investigation leads us to two conclusions: First that the earth is very large, and, secondly, that no precise estimation of its size can be deduced by observations of this kind. At an elevation of ten feet above the sea level vision is limited to a circle whose radius is about four miles, or whose area is about fifty square miles, while the whole surface is a million times as great. The highest mountains rise about five miles or about one eight-hundredth part of the radius; to conceive this slight elevation of the land imagine the earth to be reduced in size to a globe sixteen inches in diameter, then the tallest mountain would be only one one-hundredth of an inch in height—an amount scarcely perceptible to the eye. Since, then, the earth is so large, slight errors in the determination of the distance of the sea horizon are multiplied in the results, and such errors are particularly liable to occur, owing to the elevation of the visual line by the varying refraction of the atmosphere. The same objection may be made to methods founded on the measurement of the dip of the horizon, or on the vertical angles sometimes taken in geodetic surveys for the determination of the relative heights of stations.

The fact that the sum of the three angles of a geodetic triangle is greater than 180° has been mentioned as a proof that the surface of the earth is not a plane, and this may be also used, under the hypothesis of a spherical surface, to find the radius. It is proved in geometry that the areas of spherical triangles are proportional to their spherical excesses (the excess being the sum of the three angles minus 180°). Thus, if e and e' be the spherical excesses of two triangles, and A and A′ their areas,

$$\frac{e}{e'} = \frac{A}{A'}$$

Let A′ be the area of a tri-rectangular triangle whose spherical excess e' is 90°. Then, since $A' = \frac{1}{2}\pi r^2$,

$$\frac{e}{90^\circ} = \frac{2A}{\pi r^2}$$

in which e must be taken in degrees, and A and r in the same unit of measure. If e and 90° be taken in seconds, the radius is

$$r = \sqrt{\frac{2.90.3{,}600\,A}{\pi e}}$$

Hence r is known when e and A are found by observation. For example, I select two triangles of the primary triangulation of the coast of New England, recorded on page 201 of the U. S. Coast Survey Report for 1865. The first column below contains the names of the stations, the second, the measured angles, each being the mean of a great many observations, and the third the length of the sides in statute miles, each length belonging to the side opposite to the angle against which it is placed.

The sum of the three angles, given in the last line shows the observed spherical excesses to be 26″.687 and 13″.426. The areas of the triangles, being such small portions of the sphere, may be computed as if the triangles were plane and are 1,981 and 943.5 square miles respectively. Then the above formula for the radius gives, from the first triangle,

$$r=3913 \text{ miles},$$

and from the second

$$r=3962 \text{ miles}.$$

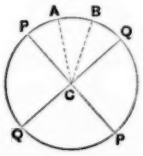
Triangle No. 84.		
Stations.	Angles.	Sides.
Gunstock.....	48° 00′ 55″.063	59.64
Thompson.....	70 37 12 .161	75.69
Wachusett.....	61 22 19 .463	70.43
	180 00 26 .687	

Triangle No. 80.		
Stations.	Angles.	Sides.
Agamenticus....	67° 35′ 59″.823	50 62
Thompson.....	61 50 53 .264	48.27
Unkonoonuc.....	50 33 20 .339	42.28
	180 00 13 426	

This method, though perhaps more accurate than that by vertical angles, is yet far from satisfactory, since the observed spherical excess is subject to errors which are multiplied in the deduced value of the radius. If, for instance, the excess in the first triangle be 26″.6, instead of 26″.687, the radius is increased by seven miles; and since the probable error of these excesses is certainly in the tenths of seconds, better methods should be sought. Thus far the only result of our discussion is that the earth, considered as a sphere, has a radius of *about* 4,000 statute miles.

Regard now the earth from an astronomical point of view, as a globe revolving on an axis from west to east every twenty-four hours, and giving rise to an apparent rotation of the celestial sphere in the opposite direction. The invariable stars describe apparent circles around the celestial poles, and from the measured zenith distances of these stars as they cross the meridian the geographical latitude of any place of observation may be found, by methods detailed in all the treatises on astronomy. Let QP QP in the figure represent a section cut from the earth's sphere by a plane passing through the axis, that is, a meridian section; PP representing the axis, QQ the equator, and C being the

Fig. I.



center of the section regarded as a circle. Let A and B be two places on this meridian whose latitudes have been found, (the angles ACQ and BCQ are these latitudes), then the angle ACB is known. Let also the linear distance between A and B be measured. From these data the lengths of the whole quadrant and of the radius are easily found. Thus let φ be the angle ACB in degrees, and m the distance AB in miles, then

$$\frac{m}{\varphi}=\text{miles in one degree}$$

and since the radius of a circle is equal in length to an arc containing 57.29578 degrees,

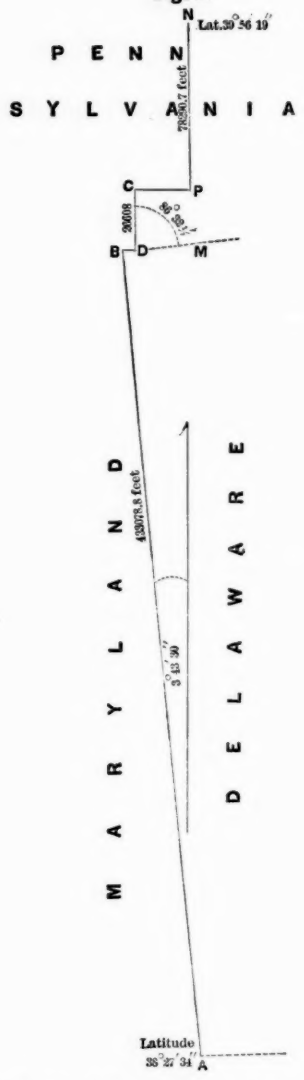
$$57.29578\frac{m}{\varphi}=\text{radius in miles}.$$

To find, then, the size of the earth, measure the distance between two points on the same meridian, and find their difference of latitude. Such, in its simplest

form, is the conception of the geodetic operation, usually called the measurement of an arc of the meridian, the successful execution of which demands the most accurate instruments, the best observers, and long continued labor. The determination of the difference of latitude is now usually made by zenith telescope observations at each station, and is perhaps the easiest part of the work. The length of the curved line of the meridian is more difficult to obtain, since it is usually impracticable to find a line of sufficient length running due north and south, and level enough to be directly measured with rods or chains. Ordinarily the two points are on different meridians, and the length of the meridian, intercepted between their parallels of latitude, is found by calculations from a triangulation carried on between them, the triangulation being itself calculated from the length of a measured base. But as a case where no triangulation is employed is the simpler, we choose such a one for the first illustration.

In the year 1763, the Penn family, proprietor of Pennsylvania and Delaware and Lord Baltimore, proprietor of Maryland, employed two surveyors or astronomers, Charles Mason and Jeremiah Dixon, to locate the boundary lines between their respective colonies. This work occupied several years, and while engaged upon it, Mason and Dixon noted that several of the lines, particularly the one between Maryland and Delaware, were well adapted to the determination of the length of a degree, being on low and level land, and deviating but little from the meridian. Representing this to the Royal Society of London, of which they were members, the latter sent tools and money to carry on the work. The measured lines are shown in the annexed sketch. AB is the boundary between Delaware and Maryland, about 82 miles long and making an angle of about four degrees with the meridian; BD is a short line running nearly east and west; CD and PN are meridians about five and fifteen miles in length respectively; CP is an arc of the parallel, the same in fact as that of the southern boundary of Pennsylvania, the real "Mason and Dixon's line" of ancient American politics. In 1766 Mason and Dixon set up a portable astronomical instrument at A, the southwest corner of the present State of Delaware, and by observing equal altitudes of certain stars, determined the

Fig. 2.



local time and the meridian, after which the azimuth of the line AB was measured, and the latitude of A found by observing the zenith distances of several stars as they crossed the meridian. At N,

a point in the forks of the river Brandywine, the zenith distances of the same stars were also measured, from which it was easy to find the latitude of N, or the difference of latitude between A and N. In 1768 they made the linear measurements by means of wooden rectangular frames 20′ long and 4′ high. All the lines had in previous years been run in the operation for establishing the boundaries, and along each of them "a visto" cut, which, says Mason on page 276 of the London Philosophical Transactions for 1768 "was about eight or nine yards wide, and, in general, seen about two miles, beautifully terminating to the eye in a point." Toward this point they sighted the rectangular frames, brought one nicely into contact with the other, made them truly level, and noted the height of the thermometer in order to correct for changes due to expansion. Through the swamps they waded with the wooden frames, but across the rivers they found the distance by a simple triangle. Thus after many wearisome weeks and months the following values were deduced and sent home to England:

Latitude of	A=38° 27′ 34″	
Latitude of	N=39 56 19	
Diff. latitude	= 1 28 45	

AB=434011.6 English feet.
BD= 1489.9 " "
DC= 26608.0 " "
PN= 78290.7 " "
Azimuth of AB at A= 3° 43′ 30″ N. W.
Angle CDB =93° 27′ 30″
CD and NP are true meridians.
CP an arc of parallel about 3 miles long.
Let us now find from these results φ the difference of latitude in degrees, and m the linear distance between the two stations A and N. The value of φ is

$$\varphi = 1.^{\circ}\ 47917$$

Now to find m project, as in the sketch below, by arcs of parallels, each line upon a meridian passing through A. Then m=AN,′ and this equals the sum of its parts N′P,′ P′D,′ D′B,′ and B′A,′ thus:

N′P′=NP=	78290.7 feet.
P′D′=CD=	26608.0 "
D′B′=DG=	89.8 "
B′A′=	433078.8 "
m=	538067.3 feet.

Here D′B′ or DG is found from the triangle BDG, taking it as plane, since its longest side is only 1490 feet long. But in finding AB′ from the triangle BAB′ where two of the sides are more than 80 miles long, AB and AB′ are considered as arcs of great circles, and B′B as an arc of a small circle of the sphere; to do this by the rules of spherical trigonometry involves a knowledge of the radius of the sphere, the very thing required to be found; but it is evident that

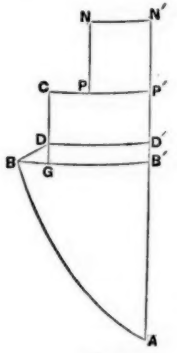


Fig. 3.

only an approximate value is needed, and a few trials will show that the result for B′A will come out the same within a small fraction of a foot, whether the radius of the earth be taken as 3800, 4000 or 4200 miles. The length of one degree of the meridian now is

$$\frac{m}{\varphi} = 363764 \text{ feet} = 68.8945 \text{ miles},$$

from which we find the value

$$r = 3947.4 \text{ miles}$$

as the radius resulting from Mason and Dixon's measurements. Since these were made on land elevated but slightly above the ocean, the result will not be materially lessened for a surface coinciding with the mean level of the waters of the earth.

But as you know very well, a more accurate way of determining the distance between two distant points is by a triangulation. Here a long chain of triangles is formed, all the angles of which are carefully observed. One, at least, of the sides is located in a level plain where it

may be very precisely measured by special tools, and by finding the elevation above the ocean of the ends of this base, its length, and hence the whole triangulation may be reduced to that surface. Astronomical observations are made at several of the stations to determine their latitudes and the azimuth of the sides with reference to the meridian. The office work then begins. First, from the known lengths of the measured base and the known angles, the lengths of all the sides of the triangles and the position of each station are computed. A meridian is then conceived to be drawn north and south through the triangulation as also parallels through each of the stations to meet this meridian, and the intercepted portions computed. The sum of these intercepts gives the length of the meridian between the northernmost and southernmost stations. Such operations, for instance, were carried on by French and Spanish scientists in Peru during the years 1736–40. From Cotchesqui to Tarqui, a distance of about 220 miles, they set out stations forming forty-three triangles. Two of the sides of these triangles were carefully measured several times with wooden rods, the northern one near Cotchesqui being 5259.2 toises, and the southern one near Tarqui being 5259.95 toises. From these bases and the measured angles the length of the meridian between the two extreme stations was computed, and found to be

$$m=176875 \text{ toises},$$

while from the astronomical observations the difference of latitute was

$$\varphi=3^\circ\ 7'\ 3''.5=3^\circ.11764.$$

Hence the length of one degree is

$$56728 \text{ toises}=68.702 \text{ miles},$$

and the earth's radius is

$$3936.4 \text{ miles}.$$

The toise, we must here say, parenthetically, was an old French measure, now of classic interest on account of its use in this expedition and in the surveys made for deciding on the length of the meter; it is equal approximately to 1.949 meters, or 6.3946 English feet, or 6.3942 American feet. The length of the degree and the radius resulting from the Peruvian arc, it must be mentioned, are not those of the ocean surface, since it lies on a high plateau, and the surveyors neglected to determine the elevation of their base lines.

It is now time that we should consider our subject more from a historical point of view, and attempt to give some account of the different efforts that have been made to determine the size of the earth. What the Indian or Chinese nations have thought and done we know not; mainly from Europe come all the records, and in early times from Greece alone. Anaximander (year −570) speculated on the shape of the earth and called it a cylinder whose height was three times its diameter, the land and sea being only upon its upper base, a view shared also by Anaxagoras (−460). Plato (−400) thought it a cube. But Aristotle (−340) gives good reasons for supposing it a sphere, and mentions, as also does Archimedes (−250), that geometers had estimated its circumference at 300,000 stadia. Eratosthenes (−230) seems, however, to have been the first to conceive the principles and make the observations necessary for a logical deduction of the size of the sphere. He noticed that at Syene, in Southern Egypt, the sun at the summer solstice cast no shadow of a vertical object, it being directly in the zenith, while at Alexandria, in Northern Egypt, the rays of the sun at the same time of the year made an angle with the vertical of one-fiftieth of four right angles. From this he concluded that the circumference of the earth was fifty times the distance between these two places, and this being, according to the statements of travelers, 5,000 stadia, he claimed for the whole circumference 250,000 stadia. The exact length of the stadia is now unknown, so that we cannot judge of the accuracy of his result; it is probably much too large, since Ptolemy, a learned astronomical writer, who flourished four hundred years later, mentions 180,000 stadia as the length of the circumference; yet the name of Eratosthenes will ever be honored in science as that of the originator of the method of deducing the size of the earth from a measured meridian arc. Posidonias (−90) made also similar observations between Alexandria and Rhodes, using a star, instead of the sun, to find the difference of latitude, and deduced 240,000 stadia for the circum-

ference. But this knowledge of the Grecians was all lost as their civilization declined, and for more than a thousand years Europe, sunk in intellectual darkness, made no inquiry concerning the size or shape of the earth. Only in Arabia were the sciences at all cultivated during this period. There the Caliph Almamoun summoned to Bagdad astronomers, and one of their labors was the measurement, on the plains of Mesopotamia, of an arc of a meridian by wooden rods, from which they deduced the length of a degree to be $56\frac{2}{3}$ Arabian miles—probably about 71 of our miles.

In the fifteenth century, when the first gleams of light broke in upon the darkness of the middle ages, men began to think again about the shape of the earth. Navigators began to doubt that its surface was a level plane, and here and there one, like Columbus, asserted it to be globular. In the sixteenth century, the learned accepted again the doctrine of the spherical form of the earth, and one of the ships of Magellan, after a three years' voyage, accomplished its circumnavigation. With the acceptance of this idea arose also the question as to the size of the globe, and Fernel, in 1525, made a measurement of an arc of a meridian by rolling a wheel from Paris to Amiens to find the distance, and observing the difference of latitude with large wooden triangles, from which he deduced about 57050 toises for the length of one degree. But it was not until the eighteenth and nineteenth centuries that science was able to call to its aid exact processes for executing successfully such difficult observations. In 1617 Snellius conceived the idea of triangulating from a known base line, and thus, near Leyden, he measured a meridian arc which gives 55020 toises for the length a degree. Norwood, in 1633, chained the distance from London to York and deduced 57424 toises for a degree. Picard, who was the first to use spider lines in a telescope, re-measured, in 1669, the arc from Paris to Amiens, using a base line and triangulation, and found one degree to be 57060 toises. This was the result that Newton used when making his famous calculation which proved that the moon gravitated toward the earth. In 1690–1718 Cassini carried on surveys in France, more accurate, probably, than any of the preceding ones, and in 1720 he published the following results:

Arc.	Mean Lat.	Length of 1°.
1.	49° 56′	56970 toises
2.	49° 22′	57060 "
3.	47° 57′	57098 "

and from these it appeared that the length of a degree of latitude increased toward the equator and decreased toward the poles, or, in other words, that the earth was not spherical, but spheroidal, and that the spheroid was prolate or extended at the poles. From the time men had ceased to believe in the flatness of the earth, and had begun to regard it as a sphere, their investigations had been directed toward its size alone; now, however, the inquiry assumed a new phase, and its shape came up again for discussion.

We must here interrupt the historical narrative to say a word about spheroids. A prolate spheroid is generated by an ellipse, revolving about its major axis, and an oblate spheriod by an ellipse revolving about its minor axis. The upper

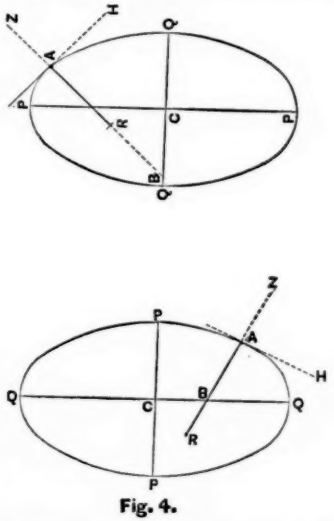


Fig. 4.

figure in this diagram represents a meridian section of the earth regarded as a prolate, and the lower shows it as an oblate spheroid. In each figure PP is the

axis, QQ the equator, C the center, A a place of observation, whose horizon is AH, zenith Z, latitude ABQ, and radius of curvature AR. Now, if the earth be regarded as a sphere and its radius be found from observations made near A, the value AR will result, it being always 57.2958 times the length of one degree of latitude at A. In the prolate spheroid the radius of curvature is least at the poles and greatest at the equator, and the reverse in the oblate. Hence if the lengths of the degrees of latitude decrease from the equator to the poles, it shows that the earth is prolate, but if they increase from the equator toward the poles it is a proof that it is oblate in shape.

Let us now go back to the year 1687, the date of the publication of Newton's Principia. In Book III of that great work are discussed the observations of Richer, who, having been sent to Cayennne, in equatorial South America, on an astronomical expedition, noted that his clock, which kept accurate time in Paris, there continually lost two seconds daily, and could only be corrected by shortening the pendulum. Now, the time of oscillation of a pendulum of constant length depends upon the force of gravity, and Newton showed, after making due allowance for the effect of centrifugal force, that the force of gravity at Cayenne, compared with that at Paris, was too small for the hypothesis of a spherical globe; in short, that Cayenne was further from the center of the globe than Paris, or that the earth was an oblate spheroid, flattened at the poles. He computed, too, that the amount of this flattening at both poles was between $\frac{1}{180}$ and $\frac{1}{300}$ of the whole diameter. Now, you will remember that Newton's philosophy did not gain ready acceptance in France; this investigation, in particular, called forth much argument, and when Cassini's surveys were completed, indicating a prolate spheroid, the discussion became a controversy. Then the French Academy resolved to send out two expeditions to make measurements of meridian arcs that would definitely settle the matter, one to the equator and another as far north as possible; for it was evident that observations near the latitude of France could afford but little information concerning the ellipticity of the meridian. Accordingly two parties sailed in 1735—Maupertius to Lapland, and Bouguer and Lacondamine to Peru. Maupertius measured his base upon the frozen surface of the river Tornea, executed his triangulation and latitude observations and returned to France in less than two years. The Peruvian expedition, whose work we have already described, was absent about seven years, but upon its return the following results could be written:

Arc.	Mean lat.	1° in toises.
Lapland..	N 66° 20′	57438
France ...	N 49° 22′	57060
Peru	S 1° 34′	56728

These figures decided the question: from this time on, every one has granted that the earth is an oblate spheroid, rather than a sphere or a prolate spheroid.

Our consideration of the earth as a sphere is not yet finished, and it cannot be here completed without anticipating to a certain extent some of the results of the following lectures. What has already been said is sufficient for us to observe that the amount of flattening at the poles, and the deviation from the spherical form is not large. In fact, on a globe sixteen inches in equatorial diameter, and on which the thickness of a coat of varnish would represent the elevation of the lands above the waters, the polar axis would be 15.945 inches, or in other words, the difference between the polar and equatorial diameters would be but one-eighteenth of an inch. It is hence evident that for many purposes it is sufficiently accurate to consider the earth as a sphere. What value then shall we take for its radius, and what is the mean length of a degree of latitude on its surface?

The mean length of a degree of latitude is the average of the lengths of all the degrees from the equator to the poles, or one-ninetieth of the elliptical quadrant. Now the following are some of the values of the length of the quadrant, according to the calculations of mathematicians, made by methods which we shall endeavor to speak of in the next lecture:

Year.	By whom.	Quadrant in Meters.
1806	Delambre	10 000 000
1819	Walbeck	10 000 268
1830	Schmidt	10 000 075
1831	Airy	10 000 976
1841	Bessel	10 000 856
1856	Clarke	10 001 515
1863	Pratt	10 001 924
1866	Clarke	10 001 887
1868	Fischer	10 001 714
1872	Listing	10 000 218
1873	Clarke	10 001 472 max.
		10 000 425 min.
1878	Jordan	10 000 681

It will be seen from this table that scientists are by no means yet able to agree upon the length of the quadrant to single meters, or tens or hundreds of meters. We select the value of Bessel, 10 000 856 meters, for two reasons, first and mainly, because this and the other dimensions of the spheroid as deduced by Bessel have been long in use in geodetic computations, and are now still in use, notwithstanding all the later investigations; and, secondly, because in regarding the earth as a sphere, it makes little difference in our results whichever value be taken (and the average of the above thirteen values is 10 000 894 meters, or nearer to Bessel's value than to any other). The mean length of one degree is then

$$\frac{10\,000\,856}{90} = 111\,121 \text{ meters.}$$

From this is deduced the following useful table of mean length of arcs of latitude:

Length of	In Meters.	In Feet.
One degree	111 121	364 556
One minute	1852	6 076
One second	30.9	101.3

The mean length of one degree in statute miles is 69.043 or $69\frac{1}{20}$. As the probable error of Bessel's value of the quadrant is about 500 meters, the probable error of the above mean length of one degree is about 55 meters or 180 feet. Stated in round numbers, easy to remember, the result is:

1° of latitude = 111.1 kilometers = 69 miles.

The mean radius of the earth, considered as a sphere, can be nothing more than the arithmetical mean or average of all the radii of the spheroid. A moment's reflection will convince us that this mean radius is the same as the radius of a sphere having a volume equal to the volume of the spheroid. Let a be the equatorial and b be the polar radius of the oblate spheroid, equal, according to Bessel to 6 377 397 and 6 356 079 meters respectively; the volume is $\frac{4}{3}\pi a^2 b$. Let r be the radius of the sphere whose volume is $\frac{4}{3}\pi r^3$. Place these values equal and we have

$$r = \sqrt[3]{a^2 b}$$

which gives

$$r = 6\,370\,283 \text{ meters,}$$

or in round numbers

$$r = 6370 \text{ kilometers,}$$

$$r = 20899 \text{ thousand feet,}$$

$$r = 3958 \text{ statute miles}$$

for the mean radius of the waters of the earth.

This mean value of r is, however, incongruous with the above mean length of a degree of latitude, since the quadrant corresponding to a radius of 6370 kilometers is nearly 6 kilometers greater than Bessel's elliptical quadrant of 10000856 meters. In some kinds of map projections it may be more logical to use the radius of a circle whose circumference is equal to the circumference of the meridian ellipse; this requires the equation

$$\tfrac{1}{2}\pi r = 10000856 \text{ meters,}$$

from which

$$r = 6366743 \text{ meters,}$$

or in round numbers

$$r = 6367 \text{ kilometers} = 3956 \text{ miles,}$$

which is less by two miles than the mean radius of the sphere. This discrepancy is unavoidable, since the properties of a sphere and an ellipsoid are not the same. At the beginning of our discussion we saw that the earth's suface could not be plane because of the discrepancies of surveys with the geome-

try of the plane, and here we see that it is also impossible, when precision is required, to consider it as spherical. Therefore, whenever in any problem, a variation of two or three miles in the length of the mean radius would make any practical change in the result of the solution it is better to regard the earth as an oblate spheroid—and this we shall discuss in the next lecture.

STEAM BOILER FURNACES FOR SMOKE PREVENTION.

By JOHN W. HILL, M. E.

Written for VAN NOSTRAND'S MAGAZINE.

The Board of Commissioners of the Cincinnati Industrial Exposition (1879) offered a cash premium of *five hundred dollars* for the best furnace system for steam boiler use, designed to burn bituminous coal without smoke.

All competing devices to be submitted to expert trials, and, to be durable, practical and capable of ready application to the prevailing types of steam boilers.

Five entries were made for the trials, as follows: The Walker Twin Furnace, by R. L. Walker & Co., of Boston, Mass: this furnace formed part of the setting of an ordinary return tubular boiler at the dry goods house of John Shillito & Co. The Fisher furnaces, by Lawrence Foulds & Fisher, of Cincinnati; this furnace formed part of the setting of a return tubular boiler at the printing establishment of Strobridge & Co.

The Eureka furnace attachment, by Douglass, Ludlow & Hart, of Cincinnati; this device was connected with the setting of a battery of two return tubular boilers at the Lane & Bodley Co. machine shops.

The Price furnace, by Wm. Price, of Cincinnati; this furnace formed part of the setting of a return tubular boiler at the Price hill inclined plane.

The Murphy furnace, by Thomas Murphy, of Detroit, Michigan; this furnace was built specially for the trials, and was set with an independent boiler on the direct and drop flue plan.

The connected boilers of all the furnaces, excepting the Murphy, furnished steam for the daily requirements of the several establishments where they were located. The steam from the Murphy boiler was blown into the atmosphere, and wasted.

THE WALKER TWIN FURNACE.

This furnace consists essentially of two independent grates, located in separate fire chambers, at opposite ends of the boiler. A pair of dampers—one at each end of the setting so arranged, that when one is closed the other is opened, and vice versa—control the direction given the gases of combustion in transit to the chimney.

The grates are alternately charged with coal, the gases of quick evolution from the freshly charged coal passing through two perforated bridge walls, and over the bed of glowing coal on the opposite grate.

The fire chamber containing the green coal, acts as a retort for the distillation of the volatile matter, which matter, as it passes through the air chamber and over the incandescent fire is oxydized and reduced to carbonic acid, or oxyd, vapor of water and sulphurous acid.

The Walker furnace at the Shillito buildings was set with a horizontal tubular boiler, furnishing steam for working the elevators, and for warming the building.

The following are the principal dimensions of furnace and boiler:

DIMENSIONS.

Boiler.	Tubular.
Length of shell	16 ft.
Diam. " "	60 ins.
Number of Tubes	48
Diam. " " (outside)	4 ins.
Heating surface shell	142.992
" " " tubes	804.249
" " " heads	16.396
" " " total	963.637
Cross section of tubes	344 sup. ft.

Furnace.	Double.
Length of fire chamber (each)	34 ins.
Width " " "	60 ins.
Grate surface, "	14.165
" " both,	28.33 sup. ft.
Heating to grate surface	34.015
Grate surface to cross-section of tubes	8.235

Grate surface to cross-section of chimney	2.555
Distance from grate to boiler	20 ins.
Bridge walls.	Perforated.
Chimney from surface of grate	148.33 ft.
Cross-section of chimney	11.09 sup. ft.

THE FISHER FURNACE.

In this furnace the ash pit is divided transversely by a brick wall built close up under the grate; Two-thirds of the ash pit being forward of the division wall. The grate is supposed to be divided into surfaces : a forward surface and a rear surface. The bars and air spaces of the forward surface are parallel to the axis of boiler, and inclined downwards seven inches in thirty-two. The rear grate is horizontal, with bars and air spaces transversely of axis of boiler. The bridge is vertical, and is built up to within seven and one-half inches of the bottom of boiler.

An air duct passes from front to rear through the bridge wall ; the opening in front, being directly under the rear grate, and the opening in the rear within two or three courses of the crest of the wall.

The opening in the rear of the bridge wall is covered with a perforated iron plate, through which the air is distributed in fine jets. Similarly perforated openings are provided in the side wall opposite the fire chamber, and aft of the bridge wall. The perforated plates in the side walls are fitted with slides to regulate the amount of opening, or close the perforations entirely.

The furnace was set with a well proportioned return flue boiler, furnishing steam to work the elevator and machinery in the printing establishment.

The following are the principal dimensions of furnace and boiler:

Boiler.	Flue.
Length of shell	24 ft.
Diam. " "	48 ins.
Number of flues	6
Diam. " " (outside)	2-10 in.
Diam. " "	4- 8 "
Heating surface shell	180.95
Heating surface flues	326.72
" " heads	11.78
" " total	519.45 sup.ft.
Cross-section of flues	2.212 " "
Furnace.	**Single.**
Length of fire chamber	50 ins.
Width " "	48 "
Grate surface	16.640 sup ft.
Heating to grate surface	31.970
Grate surface to cross-section of flues	7.523
Grate surface to cross-section of chimney	4.160
Distance from grate to boiler	17.24 ins.
Space over bridge wall	75 "
Chimney from surface of grate	58.33 ft.
Cross-section of chimney	4.00 supt. ft.

THE EUREKA FURNACE ATTACHMENT.

This device consists of a series of steam jets, surrounded by circular bell-shaped muzzles, set in the furnace front, through which air is drawn into the fire chamber by induction.

The device, as tried under a battery of two boilers, embraced eight steam nozzles, having a free orifice, .0625″ diameter and eight air nozzles, having a free orifice, 1.25″ diameter.

The steam nozzles are all connected to a horizontal pipe passing across the furnace front over the fire doors. This pipe receives steam from the boilers, the flue being regulated by an ordinary stod valve.

The furnace is unchanged in applying this attachment, except the drilling of a horizontal series of holes in the fire front for the reception of the air nozzles.

The boilers' return tubular furnished steam for the foundry and machine shop of the Lane & Bodley Co., and were set in the manner common to their class.

The following are the principal dimensions of furnace and boilers :

Boilers.	Tubular.
Length of shell	16 ft.
Diameter of shell	38 ins.
Number of tubes, (each boiler)	21
Diameter of tubes, (outside)	4
Heating surface shells	160.000
" " tubes	703.718
" " heads	16.446
" " total	880.158 sup.ft
Cross section of tubes	3.010 "
Furnace.	**Single.**
Length of fire chamber	48 ins.
Width " "	72 "
Grate surface	24 sup. ft.
Heating to grate surface	36.673
Grate surface to cross section of tubes	7.973
Grate surface to cross section of chimney	2.396
Distance from grate to boiler	17 ins.
Space over bridge wall	7 "
Chimney from surface of grate	57.34 ft.
Cross section of chimney	10.017 sup. ft.

THE PRICE FURNACE.

This furnace consists of two parallel fire chambers and grate surfaces, made by dividing the usual fire chamber with a longitudinal wall, built from the floor of the ash-pit, close up to the boiler. Near the forward end the wall is perforated with at throat, to form a connection between the two fire chambers. The gases of quick evolution from the green coal of one grate pass through the throat and over the incandescent coke on the opposite grate in transit through the furnace.

The division wall extends back to the bridge wall, which, in this furnace, is built close up to, and with an inverted arch embraces the bottom of the boiler.

Through the lower portion of the bridge wall are cut two flues or openings, capable of being alternately closed and opened by a sliding damper. By a proper working of the damper and charging of the coal, the two fire chambers are alternately made to act as retorts. The volatile matter from the freshly charged coal passes forward in the fire chamber, where it is distilled through the throat and back over the coke on the opposite grate.

The gases of combustion from both fire chambers pass back through the open flue in the bridge wall, thence through the furnace and tubes of the boiler in the usual manner.

The following are the principal dimensions of furnace and boiler:

Boiler.		Tubular.
Length of shell		16 ft.
Diameter of shell		54.5 ins.
Number of tubes		40
Diameter of tubes, outside		4 ins.
Heating surface:	Shell	136.976
" "	Tubes	670.208
" "	Heads	15.866
" "	Total	823.050 sup. ft.
Cross-section of tubes		2.867 "

Furnace.		Double.
Length of fire chambers (each)		60 ins.
Width " " "		27 "
Grate surface, each		11.25
" both		22.50 sup. ft.
Heating to grate surface		36.58 "
Grate surface to cross-section of tubes		7.848 "
Grate surface to cross-section of chimney		4.282 "
Distance from grate to boiler		23.27 ins.
Bridge wall.		Perforated.
Chimney from surface of grate		88 ft.
Cross-section of chimney		5.245 sup. ft.

THE MURPHY FURNACE.

This furnace consists principally of an independent oven, set forward of the boiler (after the manner of furnaces for burning spent tan from the leach), in which combustion is completed before the gases are permitted to impinge upon the heating surfaces of the boiler.

In the other furnaces the bottom of the boiler forms the roof of the fire chamber. In this furnace the roof of the fire chamber is a fire-brick arch of large radius.

The grate bars are single and set to form a V-shaped fire-bed, similar to the well-known Waddington grate. The grate is divided into two distinct surfaces on the axial line of fire chamber, each section of bars being mounted at the inner end on a revolving shaker shaft, operated from the front, for the removal of clinkers and ash.

The two sections of the grate are separated at the inner ends, about six inches, for the reception of a toothed clinker bar, the squared end of which is projected through the furnace front, and worked with a socket wrench.

Upon each side of the oven a hopper or magazine is placed for the reception of finely broken coal, which is fed on the grate by a reciprocating plunger sliding under the mouth of the hopper, and worked by a rock shaft from the front of the furnace.

All coal charged to the grate is first passed through the two hoppers, from which it is delivered in a partially coked state, and in small charges.

The sharp inclination of the grate bars precipitates nearly if not quite all the fusible and vitrifying matter in the coal upon the clinker bar, from which it is readily removed as desired by a partial rotation of the bar.

No hand stroking of the fire is necessary with this furnace; the entire manipulation of the coal, after it is charged into the magazines, being accomplished from the front, by the devices already mentioned.

The rear end of the oven terminates in a throat, through which the gases of combustion pass into the tubes of the boiler. The boiler direct tubular was set on the drop flue plan, the whole tendency of the hot gas being from above downwards.

A system of water heating tubes was connected to the under side of the boiler, into which the feed water was introduced direct from the pump, and around which the hot gas circulated just before passing into the chimney.

The following are the principal dimensions of furnace and boiler:

Boiler.		Tubular.
Length of shell		8 ft.
Diameter of shell		36 ins.
Number of fire tubes		30
Diameter " " (outside)		3 ins.
Number of water tubes		49 ins.
Length of water tubes		8 ft.
Diameter " " (inside)		1 in.
Heating surface shell	32.987	
" " fire tubes	188.496	
" " heads	3.686	
" " water tubes	102.626	
" " total	327.795	sup.ft.
Cross-section of fire tubes	1.127	"

Furnace		Single.
Length of furnace		36 ins.
Width " "		42 "
Grate surface	10.50	sup.ft.
Heating to grate surface	31.22	"
Grate surface to cross-section of fire tubes	9.313	"
Great surface to cross-section of chimney	9.822	"
Chimney from surface of grate	40 ft.	
Cross-section of chimney	1.069	sup.ft.

Pittsburgh coal was chosen for the trials for several reasons: First; the high percentage of volatile matter and facility with which it is distilled from the fuel renders it a difficult coal to work in furnaces designed for smoke prevention, and a furnace capable of working this coal successfully (in the matter of smoke prevention) can be relied upon to furnish equivalent if not better results with any other coal: Second; Pittsburgh coal contains a higher percentage of combustible and a higher thermal value per unit of combustible, than any other known coal; and economic results obtained from this coal exhibit the maximum efficiency of furnace, under the conditions of trial: Third; Pittsburgh coal is well-known wherever bitumenous coal is used in the United States, and the results of trials with this coal, can be easily compared with the results of many former trials reduced to a Pittsburgh coal basis.

The coal used for the trials was obtained from the yards of Marmet & Co (Cincinnati) and was of excellent quality. From the analysis for the purpose of the trials by Prof. Bruno Kniffler, and the known distribution of heat in the trial of the Murphy furnace, the thermal value of the combustible has been taken at 15500 units.

The coal was weighed to all the furnaces excepting the Murphy in uniform charges of 200 pounds.

The comparatively small dimensions of the Murphy furnace and boiler, and low actual rate of coal consumption, suggested the propriety of uniform charges of 100 pounds, in this instance.

The charges (time and weight) of coal were noted and checked independently by two observers, and no charge was permitted to be removed from the scale until both observers were satisfied as to the weight, and had entered the charge in their note books.

At the end of trial, after the fire was restored to its original condition under the direction of the writer, the unburnt coal was weighed back and deducted from the total quantity charged.

The fire and ash pit having been carefully cleaned at commencement of trial, all ash and clinker on the grate and in the ash pit at end of trial was weighed back dry, and the difference between this weight and the net weight of coal charged, is held to represent the weight of combustible fired.

Of the weight of ash and clinker returned, was a small percentage of combustible, which worked through the grate, the value of which is obtained by deducting from the observed weight of non-combustible—the weight of non-combustible, as determined by analysis.

Each competitor had complete control of his coal and furnace during the trial.

The water delivered to the boilers was measured in a tight tank divided into two compartments, one compartment containing 1728.5 pounds, and the other 1727.0 pounds with water at 70 Fahr. In the upper edge of the partition dividing the tank, a notch was cut with beveled edges. The compartments of the tank were alternately filled to the crest of the dividing partition, from the city mains, and drawn down to the lower edge of the outlet pipe in the side of the tank. Uniform volumes of water were delivered by the measuring tank for all the trials.

From either compartment of the measuring tank, the water was drawn into a supplemental tank, connected with the feed pump. The level of water in the supplemental tank being carefully noted at commencement of trial, the same level obtained at end of trial, and the number of full tanks charged together with the final partial tank, held to represent the total delivery of water to the boilers during the trial.

To determine the thermal value of the steam furnished by the boiler a small quantity was drawn through a calorimeter and condensed. The condensation was collected in a tight tin can and periodically weighed. The water expended in condensing the steam was measured into a tight barrel through a Worthington meter.

From the barrel the water entered the condenser at the bottom at a normal temperature, and passed out at the top at an elevated temperature: the elevation of temperature being due, the heat transferred from the steam to water, which was all it contained, except the small quantity resident in the condensation as it flowed from the end of the worm. The temperatures of the condensing water as it entered and left the condenser, and the temperature of the condensation as it left the worm, were read to quarter degreee regularly every fifteen minutes.

The observations of smoke issuing from the chimney were taken regularly even seven and one half minutes during the trial, except in cases when the darkness near the end of trial prevented an accurate reading of the chimney.

In reading the chimney, the following arbitrary code governed the observers.

The entire absence of smoke was taken at 100, indicating the best possible smoke-prevention. Faint traces of smoke in the waste gases was taken at 90, indicating a result sometimes obtained with an excellent construction of furnace and a skillful manipulation of the fire. Discoloration of the waste gases, readily perceptible, was taken at 75, indicating a state of smoke-prevention above the average of furnace performance. Ordinary smoke issuing from the chimney was taken at 50. Fairly black smoke was taken at 30, and thick, dense, black smoke was taken at 10.

The trials were made upon the following dates:

Walker	furnace	Oct. 1st.
Fisher	"	" 4th.
Eureka	" attachment	" 7th.
Price	"	" 10th
Murphy	"	" 14th

The trials were limited to ten hours each, by reason of all the furnaces, excepting the Murphy, being in manufacturing establishments, where night runs would have been objected to.

The duration of trials was as follows:

Walker	8:30	A. M. to	6:30	P. M.
Fisher	8:00	" "	6:00	"
Eureka	8:15	" "	6:15	"
Price	8:45	" "	6:45	"
Murphy	8:30	" "	6:30	"

The steam pressures were read from a Bourdon gage which had been carefully prepared for the trials; and the following are means of forty-one readings:

STEAM PRESSURES.

Walker	38.755 Pds.
Fisher	80.287 "
Eureka	76.181 "
Price	82.100 "
Murphy	81.575 "

The temperature of air, water from City mains, and feed to the boiler, were taken with Green and Tagliabue thermometers: and the temperature of feed when water was heated by exhaust steam from the connected engine, was taken in the feed pipe close to the check valve. The Walker and Murphy boilers were unprovided with feed heating apparatus, and took water direct from the supplemental tank. The following are means of forty-one observations:

TEMPERATURE OF AIR.

Walker	100.07 Fahr.
Fisher	83.27 "
Eureka	82.21 "
Price	83.46 "
Murphy	80.28 "

TEMPERATURE OF WATER FROM CITY MAINS.

Walker	70.025 Fahr.
Fisher	71.069 "
Eureka	72.640 "
Price	73.569 "
Murphy	74.550 "

TEMPERATURE OF FEED.

Walker	70.025 Fahr.
Fisher	166.012 "
Eureka	169.112 "
Price	176.360 "
Murphy	74.550 "

The pressure of atmosphere was read from a compensated aneroid barometer, and the following are means of forty-one observations:

Walker	29.760	Ins.
Fisher	29.693	"
Eureka	29.630	"
Price	29.334	"
Murphy	29.156	"

The temperature of waste gases was taken with a high range centigrade thermometer. In the front connection, near the chimney, a hole was cut for the reception of an iron tube, closed at the lower end, in which the thermometer was suspended with a bath of linseed oil. With the exception of the Fisher trial, where the oil boiled over several times, the following are the means of forty-one observations:

TEMPERATURE OF WASTE GASES.

Walker	541.29	Fahr.
Fisher	542.28	"
Eureka	468.30	"
Price	423.05	"
Murphy	279.67	"

The temperatures of hot gas in the fire chamber and in the back connection were taken by calorimeter process: pieces of one inch bar iron, six inches long, were drilled axially for the introduction of a steel rod, upon which the wrought iron bar was mounted and thrust into the center of the fire chamber and back connection through apertures cut in the brick work. The iron rods were made to weigh precisely *one* pound and to fit the steel rod loosely. Into a pail ten pounds of water was carefully weighed; the iron rods, being allowed to remain in the furnace a sufficient length of time to acquire the temperature of the enveloping hot gas, they were carefully withdrawn and dropped into the pail. The known weights of iron and water and elevation of temperature of water, together with the specific heat of wrought iron, constitute the elements of the calculations. Thetempera tures of water were taken with a Green thermometer, and the specific heat of the iron rods was taken at .1139. The actual specific heat of water at observed temperatures was neglected, and the specific heat uniformly taken as 1.0000. The following are means of ten set of observations during each trial.

TEMPERATURE OF FIRE.

Walker	chamber with incandescent fire	1931.24	Fahr.
	chamber with green fire	1486.68	"
Fisher		1902.94	"
Eureka		2369.30	"
Price	chamber with incandescent fire	3053.00	"
	chamber with green fire	2491.07	"
Murphy		3056.75	"

TEMPERATURE BACK END.-

Walker	See green fire.	
Fisher	665.12	Fahr.
Eureka	1043.15	"
Price	1587.92	"
Murphy	812.35	"

To determine the amount of heat expanded in elevating the temperature of the vapor of water in the air hygrometer, readings were made half hourly during each trial, with the following results, as means of twenty-one observations.

HYGROMETER.

	Air.	Dew Point.	Dryness.
Walker	97.65	84.83	12.82
Fisher	83.82	71.54	12.28
Eureka	81.52	69.85	11.68
Price	83.34	75.15	8.19
Murphy	80.70	72.18	8.52

The calorimeter data, from which the quality of steam furnished is calculated, have been averaged for each trial, and are given in the following table;

INITIAL TEMPERATURE, CONDENSING WATER.

Walker	74.09	Fahr.
Fisher	73.94	"
Eureka	73.43	"
Price	76.40	"
Murphy	74.37	"

FINAL TEMPERATURE, CONDENSING WATER.

Walker	87.66	Fahr.
Fisher	86.14	"
Eureka	81.42	"
Price	97.22	"
Murphy	114.96	"

THERMAL UNITS PER POUND OF CONDENSING WATER.

Walker	13.57	Fahr.
Fisher	12.20	"
Eureka	7.99	"
Price	20.82	"
Murphy	40.59	"

TEMPERATURE OF CONDENSATION.

Walker	85 43	Fahr.
Fisher	83.40	"
Eureka	79.00	"
Price	85.71	"
Murphy	77.08	"

CONDENSING WATER, PER LB. OF STEAM.

Walker....	3288.75 water / 51 steam	... 64.483	Pounds.
Fisher.....	10041.53 water / 139 steam	... 72.240	"
Eureka....	6633.61 water / 57.18 steam	...116.012	"
Price.....	6362.82 water / 93.25 steam	... 68.233	"
Murphy ...	5564.70 water / 165.56 steam	... 33.611	"

The utter unreliability of boiler efficiency, based upon the water pumped into the boiler, and the coal fed on the grate, was never better shown than by these trials. None of the boilers were hard worked, and without data to the contrary, it would naturally be supposed that the low rate of evaporation per unit of heating surface, and low rate of coal consumption per unit of grate surface, would guarantee saturated steam.

In the following table the heat per pound of steam is based upon the total water fed to boiler, or rather upon the proportion of that water as evaporation diverted to the condenser. In the Walker, Fisher and Eureka boilers a material portion of the water pumped in was entrained in the steam, producing a mean thermal value per pound of steam considerably less than that of saturated steam at observed pressures. Upon the other hand, the Price and Murphy steam exhibits a very high super-heat.

TOTAL HEAT PER POUND OF STEAM CONDENSED.

Walker.....	960.46 units.
Fischer.....	964.73 "
Eureka.....	1005.93 "
Price	1506.32 "
Murphy.....	1441.35 "

The air entering the furnace per pound of combustible has been calculated from the known temperature of fire, thermal value of the combustible and mean specific heat of the gases of combustion.

The mean observed temperature of fire in the Walker furnace was 1931.24, and taking the mean specific heat of the gases of combustion at .238; then the weight of hot gas per pound of combustible becomes

$$\frac{15{,}500}{(1931.24-100.07).238} = 35.565 \text{ pounds};$$

of this quantity one pound was combustible from the coal.

The mean observed temperature of fire in the Fisher furnace was 1902.94; and the weight of hot gas per pound of combustible

$$\frac{15{,}500}{(1902.94-83.27).238} = 35.789 \text{ pounds};$$

of this quantity one pound was combustible from the coal.

The mean observed temperature of fire in the Eureka furnace was 2369.30; and weight of hot gas per pound of combustible

$$\frac{15{,}500}{(2369.30-82.21).238} = 28.475 \text{ pounds};$$

of this quantity one pound was combustible from the coal.

The mean observed temperature of fire in the Price furnace was 3053.00; and weight of hot gas per pound of combustible

$$\frac{15{,}500}{(3053-83.46).238} = 21.931 \text{ pounds};$$

of this quantity one pound was combustible from the coal.

The mean observed temperature of fire in the Murphy furnace was 3056.75; and weight of hot gas per pound of combustible

$$\frac{15{,}500}{(3056.75-80.28).238} = 21.880 \text{ pounds};$$

of this quantity one pound was combustible from the coal.

AIR PER POUND OF COMBUSTIBLE.

Walker.....	34.565 pounds.
Fisher.....	34 789 "
Eureka.....	27.475 "
Price.....	20.931 "
Murphy.....	20.880 "

The weight of vapor of water in the air supporting combustion is stated in decimal of a pound per pound of air supplied.

VAPOR OF WATER IN THE AIR.

Walker.....	.02314
Fisher.....	.01556
Eureka.....	.01485
Price.....	.01787
Murphy.....	.01612

In the following table are given the total quantities of coal charged, ash and clinker returned, water pumped into boilers, per centage of net coal charged, utilized as combustible, and combustible in ash in per centage of net coal charged.

COAL CHARGED.

Walker	3800 pounds.
Fisher	1866 "
Eureka	3420 "
Price	2694 "
Murphy	779 "

ASH, CLINKER AND COMBUSTIBLE RETURNED.

Walker	202.00 pounds.
Fisher	87.50 "
Eureka	113.00 "
Price	119.00 "
Murphy	28.75 "

PER CENTAGE OF COMBUSTIBLE.

Walker	94.685
Fisher	95.365
Eureka	96.692
Price	95.582
Murphy	96.306

COMBUSTIBLE IN ASH.

Walker	2.273
Fisher	1.593
Eureka	0.262
Price	1.376
Murphy	0.652

WATER TO BOILERS.

Walker	28859.00 pounds.
Fisher	10896.00 "
Eureka	33133.77 "
Price	23282.92 "
Murphy	6854.50 "

EVAPORATION.

The apparent evaporation per pound of coal with the Walker furnace and boiler was $\frac{28859}{3800}=7.5946$ pounds; but the mean heat per pound of steam condensed in the calorimeter was 960.46 units, and total heat per pound of coal in the steam was $7.5945\times960.46=7294.21$ units. Of this quantity $70.025\times7.5945=513.8$ units were in the feed water, and the evaporation per pound of coal from and at a temperature of 212 Fahr., becomes

$$\frac{7294.21-513.8}{966}=7.019 \text{ pounds.}$$

The combustible was .94685 of net coal charged and evaporation per pound of combustible, from and at 212 Fahr. was,

$$\frac{7.019}{.94685}=7.413 \text{ pounds.}$$

The apparent evaporation per pound of coal with the Fisher furnace and boiler was $\frac{10896}{1866}=5.839$ pounds; but the mean heat per pound of steam condensed in the calorimeter was 964.73 units, and total heat per pound of coal in the steam was

$$5.839\times964.73=5633.058 \text{ units.}$$

Of this quantity

$$166.012\times5.839=969.34 \text{ units}$$

were in the feed water, and the evaporation per pound of coal from and at a temperature of 212 Fahr. was,

$$\frac{5633.058-969.34}{966}=4.828 \text{ pounds.}$$

The combustible was .95365 of net coal charged and evaporation per pound of combustible, from and at 212 Fahr. was,

$$\frac{4.828}{.95365}=5.062 \text{ pounds.}$$

The apparent evaporation per pound of coal with the Eureka furnace and boiler was $\frac{33133.77}{3420}=9.688$ pounds; but the mean heat per pound of steam condensed in the calorimeter was 1005.93 units, and total heat per pound of coal in the steam was

$$1005.93\times9.688=9745.45 \text{ units.}$$

Of this quantity, $169.112\times9.688=1638.357$ units were in the feed water, and evaporation per pound of coal from and at 212 Fahr., was

$$\frac{9745.45-1638.357}{966}=8.392 \text{ pounds.}$$

The evaporation by the Eureka boiler should be reduced by the amount of steam eqpended in maintaining the jets. The orifices in the nozzles were .0625″ diameter, and area of eight nozzles

$$\frac{8.\times.0625^2\times.7854}{144}=.00017 \text{ sup. ft.}$$

Considering the form of the nozzle, and approach thereto, it is probable that the velocity of flow referred to full area of orifice, was about 1,000 feet per second, from which is obtained the weight of steam expended per hour in the jets, as

$$3600\times.00017\times1000\times.2139=131.33 \text{ pounds,}$$

and per centage of steam absorbed by the device.

$$\frac{131.33}{2545.}=.0516^{*}$$

From which is deduced the net evaporation per pound of coal from, and at 212 Fahr., as

$$.9484\times 8.392=7.957 \text{ pounds.}$$

The combustible was .96692 of net coal charged, and evaporation per pound of combustible from and at 212 Fahr., was

$$\frac{7.959}{.96692}=8.231 \text{ pounds.}$$

The apparent evaporation per pound of coal with the Price furnace and boiler was

$$\frac{23282.92}{2694}=8.6425 \text{ pounds;}$$

but the mean heat per pound of steam condensed in the calorimeter, was 1506.32 units, and total heat per pound of coal in the steam was

$$1506.32\times 8.6425=13018.37 \text{ units.}$$

Of this quantity,

$$176.36\times 8.6425=1524.17 \text{ units}$$

were in the feed water, and the evaporation per pound of coal from and at 212 Fahr., was

$$\frac{13018.37-1524.17}{966}=11.8988 \text{ pounds.}$$

The combustible was .95582 of net coal charged, and evaporation per pound of combustible from and at 212 Fahr., was

$$\frac{11.8988}{.95582}=12.449 \text{ pounds,}$$

The apparent evaporation per pound of coal with the Murphy furnace and boiler was

$$\frac{6854.5}{779}=8.7991 \text{ pounds.}$$

but the mean heat per pound of steam condensed in the calorimeter was 1441.35 units, and total heat per pound of coal was

$$1441.35+8.7991=12.682.58 \text{ units.}$$

Of this quantity,

$$74.55+8.7991=655.973 \text{ units}$$

were in the feed water, and the evaporation per pound of coal from and at 212 Fahr., was

$$\frac{12682.58-655.973}{966}=12.4499 \text{ pounds.}$$

The combustible was .96306 of net coal charged, and evaporation per pound of combustible from and at 212 Fahr., was

$$\frac{12.4499}{.96306}=12.9274 \text{ pounds.}$$

CAPACITY OF BOILER.

The rate of evaporation per superficial foot of heating surface per hour, from and at 212 Fahr. was for the Walker boiler,

$$\frac{7.019\times 380}{963.637}=2.7679 \text{ pounds.}$$

For the Fisher boiler,

$$\frac{4.828\times 186.6}{519.45}=1.7343 \text{ pounds.}$$

For the Eureka boiler

$$\frac{7.959\times 342}{880.158}=3.2062 \text{ pounds.}$$

For the Price boiler,

$$\frac{11.8988\times 269.4}{823.05}=3.8947 \text{ pounds.}$$

For the Murphy boiler,

$$\frac{12.4499\times 77.9}{327.795}=2.9587 \text{ pounds.}$$

RATE OF COMBUSTION.

The coal charged per superficial foot of grate, per hour, was for the Walker furnace,

$$\frac{380}{28.33}=13.413 \text{ pounds.}$$

For the Fisher furnace,

$$\frac{186.6}{16.64}=11.214 \text{ pounds.}$$

For the Eureka furnace,

$$\frac{342}{24}=14.25 \text{ pounds.}$$

For the Price furnace.

* In the report of this trial to the Commissioners of the Exposition, an error occurs in stating the fraction of total steam expending in maintaining the jets. The calculation resulting in 131.33 pounds, supposes steam *only* to pass the orifices. The error lay in overlooking the fact that the percentage of water entrained in the steam, passed through the calorimeter, would also apply to the steam passed by the jets. In other words, the denominator of the fraction representing the steam expended in maintaining the jets should have been the net evaporation per hour instead of the water per hour pumped into the boilers. The water entrained

$$\frac{.7681}{(23.19 \text{ per cent.})}$$

had a relative volume of $291.66.\frac{.7681}{2319}=965$ or 1 per cent. of the volume of steam discharged.

$$\frac{269.4}{22.50}=11.973 \text{ pounds.}$$

For the Murphy furnace,

$$\frac{77.9}{10.5}=7.419 \text{ pounds.}$$

RESUME OF RESULTS.

Steam per pound of coal from and at 212 Fahr.

Walker	7.019	pounds.
Fisher	4.828	"
Eureka	7.959	"
Price	11.898	"
Murphy	12.450	"

Steam per pound of combustible from and at 212 Fahr.

Walker	7.413	pounds.
Fisher	5.062	"
Eureka	8.231	"
Price	12.449	"
Murphy	12.927	"

Steam per sup. foot of heating surface per hour.

Walker	2.7679	pounds.
Fisher	1.7343	"
Eureka	3.2062	"
Price	3.8947	"
Murphy	2.9587	"

Coal per sup. foot of grate surface per hour.

Walker	13.413	pounds.
Fisher	11.214	"
Eureka	14.250	"
Price	11.973	"
Murphy	7.419	"

SMOKE PREVENTION.

In comparing results in the following table it should be observed, that absolutely no smoke was rated 100, and the ordinary condition of chimney gases was taken at 50:

Walker	86.923
Fisher	80.125
Eureka	86.140
Price	81 202
Murphy	98.467

THE DISTRIBUTION OF HEAT.

Specimen lumps of coal were taken from the several lots furnished the competitors in the trials, and submitted to analysis by Prof. Bruno Kniffler, with the following result:

Fixed carbon	61.038
Volatile matter	32.750
Sulphur	0.863
Moisture	2.307
Ash	3.042
	100.000

As already stated, the thermal value of the combustible has been taken at 15,500 units: equivalent to an evaporation from and at 212 Fahr. of 16.045 pounds.

WALKER FURNACE.

	Thermal units.	Steam.	Per centage.
Steam	7161.155	7.413	46.201
Chimney gas	3734.725	3.866	24.095
Vapor of water	169.415	0.175	1.093
Moisture in coal	30.070	0.031	0.194
Combustible gas	775 000	0.802	5.000
Radiation	3629.635	3.758	23.417
	15500.000	16.045	100.000

FISHER FURNACE.

	Thermal units.	Steam.	Per centage.
Steam	4889.940	5.062	31.548
Chimney gas*	7710.320	7.982	49.744
Vapor of water	238.700	0.247	1.540
Moisture in coal	30.225	0.031	0.195
Combustible gas	1085.000	1.123	7.000
Radiation	1545.815	1.600	9.973
	155000.000	16.045	100.000

EUREKA FURNACE.

	Thermal units.	Steam.	Per centage.
Steam	8384 570	8.679	54.094
Chimney gas	2616.555	2.709	16.881
Vapor of water	75.640	0.078	0.488
Moisture in coal	28.985	0.030	0.187
Combustible gas	620.000	0.642	4.000
Radiation	3774.250	3.907	24 350
	15500.000	160.45	100.000

PRICE FURNACE.

	Thermal units.	Steam.	Per centage.
Steam	12026.140	12.449	77.588
Chimney gas	1772.890	1.835	11.438
Vapor of water	60.295	0.063	0.389
Moisture in coal	28.830	0.030	0.186
Combustible gas	387.500	0.401	2.500
Radiation	1224.345	1.267	7.899
	15500.000	16.045	100.000

MURPHY FURNACE.

	Thermal units.	Steam.	Per centage.
Steam	12488.195	12.927	80.569
Chimney gas	1033.075	1.069	6.665
Vapor of water	32.085	0 034	0.207
Moisture in coal	26.970	0.028	0.174
Combustible gas	387.500	0.401	2.500
Radiation	1532.175	1.586	9.885
	15500.000	16.045	100.000

In the tables above the first four quan-

* The air openings in the bridge wall, and in the side walls of the Fisher furnace, behind the bridge wall, are equal to the effective area of openings in front of the bridge wall, and the weight of hot gas per pound of combustible passing up the chimney was twice the weight of air per pound of combustible, plus one.

tities are calculated from known data. No analysis having been made in either of the trials, of the chimney gases; the percentage, or thermal value per pound of combustible of the combustible gas lost in the chimney is not known.

A careful comparison of the data, with that of previous trials when the chimney gases have been analyzed, suggests the values given to the combustible gas in the tables; and the heat lost by conduction and radiation is taken as the difference between the heat accounted for and the total heat of combustion.

EXPERIMENTS ON THE FILTRATION OF WATER.

By GEORGE HIGGIN, M. Inst. C. E.

From the Proceedings of the Institution of Civil Engineers.

In the autumn of the year 1877, the author, being in Buenos Ayres, had occasion to make some experiments on the filtration of the water with which it was proposed to supply that city. The works for that purpose have been designed by, and are being executed under the direction of Mr. J. F. Bateman, President Inst. C. E., the water being drawn from a deep channel of the River Plate almost in front of the town or suburb of Belgrano, and at a point about four miles above the most northerly extremity of the city.

The River Plate is formed by the junction of the rivers Paraná and Uruguay, about twenty miles above the city of Buenos Ayres. The united streams, from this point down to where they discharge into the ocean at Monte Video, bear the name of Rio de la Plata, or Silver river, translated by English people into River Plate. The total length of the River Plate is only about 120 miles; but it has a width at its mouth of 60 miles, whilst at Buenos Ayres, 100 miles from its mouth, it has a width of 30 miles. It is extremely shallow, the deep water channel seldom exceeding 22 or 24 feet in depth, whilst on the banks, which are numerous and of great extent, the depth is much less.

As stated by Mr. Bateman, in his report on the proposed "Improved Harbor Accommodation" of Buenos Ayres, 1871, the mean low-water discharge of the River Uruguay may be taken at 150,000 cubic feet per second, and that of the Paraná at 520,000 cubic feet per second. The mean low water discharge of the River Plate would be, therefore, 670,000 cubic feet per second. The water of the Uruguay is, under ordinary circumstances, clear, free from detritus, and of a good class. A sample, from the center of the river in front of Fray Bentos, on the 9th of September, 1877, when the river was high, gave, under analysis, the following result:

	Parts per Million.
Total solid residue.....	86.00
Chlorine.....	1.70
Free ammonia.....	0.03
Albumenoid ammonia...	0.20

Hardness, 1.4° per 100,000.

This water was almost clear, and threw down a very slight deposit when left in repose. Another sample, from the middle of the river in front of Higueritas, a little lower down, taken on the following day, gave, under analysis, the following result:

	Parts per Million.
Total solid residue.....	87.00
Chlorine.....	2.00
Free ammonia.....	0.00
Albumenoid ammonia..	0.14

A sample taken from the center of the river in front of the town of Concordia, and about 250 miles above its junction with the Paraná, contained only thirty-nine parts per million of total solids, and was therefore much purer than the water at Fray Bentos and Higueritas. The composition of this water was, however, curious, for out of the thirty-nine parts no less than 18.5 were composed of silicic acid, and the water contained no chlorine. Probably no other river water in the world contains such a large percentage of silicic acid, and is so free from chlorides.

The water of the Paraná is very different in composition from that of the Uruguay. The Paraná is a delta-forming river; its waters are always, even in the driest season, highly colored and opaque. In dry seasons the detritus in suspen-

sion is in so finely divided a state, that months of repose will not render the water clear, nor can it be cleared by passing through several folds of filter paper. In the wet season, or when the river comes down charged with the waters from the tropical rains of the interior, it is very turbid.

Mr. Bateman states, that at the time of his examination of the river Paraná in the autumn of 1871, "the total quantity of matter held in suspension..... was but $\frac{1}{1000}$ part by weight;" and that "this amount, will be, no doubt, increased in floods." Some experiments by the author in 1877, on the water of the River Plate, in front of Buenos Ayres, showed that it then contained $\frac{1}{1152}$ part by weight of suspended matter. The river was probably at that time in a more turbid condition than when examined by Mr. Bateman in 1871, though it could not be called "in flood." Assuming a mean flow of 700,000 cubic feet per second, which is a little more than its minimum volume, the river at that time was carrying seawards, every twenty-four hours in round numbers, 224,000 tons of sediment, or about 82,000,000 tons per annum—an enormous mass of material, which fully accounts for the deltaic character of the River Plate.

The Paraná, down to the town of San Pedro, which lies about seventy miles above its discharge into the Plate, continues in one stream. At this point it splits into two main channels; one of which, known as the Paraná de las Palmas, continues a nearly straight course down to its mouth, about twenty miles above Buenos Ayres; whilst the other, known as the Paraná Guazu, trends off in a northerly direction, and flows into the Plate in two channels, almost at the same point where the Uruguay unites with it. The triangle, formed between the town of San Pedro at the apex and the points of discharge of these two principal channels some twenty miles apart, is a delta intersected by various branches of the main streams, which wind about and cross each other in a most intricate manner. The water which flows past Buenos Ayres is probably almost entirely that coming down the Palmas branch of the Paraná.

Two comparatively small rivers, the Lujan and Tigre, join the River Plate about eighteen miles above Buenos Ayres; and although their volume is small in proportion to the Palmas branch of the Paraná, from their position on the right bank, and discharging so near the town, they probably influence the character of the water at Buenos Ayres.

According to analysis made in 1872 by Professor Kyle, of the National College of Buenos Ayres, the water of the Palmas at four leagues from its mouth, contained:

	Parts per Million.
Total solid residue.....	100.00
Chlorine.....	15.00
Free ammmonia.....	0.16
Albumenoid ammonia..	0.24

Hardness, 3.4°.

The water of the Lujan, according to an analysis, by the author, of a sample taken on the 4th of October, 1877, yielded:

	Parts per Million.
Total sold residue.....	1,671.00
Chlorine.....	160.50
Free ammonia.....	0 014
Albumenoid ammonia..	0.28

Hardness, 15.75°.

The water of the Tigre, according to a sample taken on the same date, and analyzed by the author, contained:

	Parts per Million.
Total solid residue.....	898.76
Chlorine.....	99.90
Free ammonia.....	Traces
Albumenoid ammonia..	0.11

Hardness, 12°.

The water in front of the town of Buenos Ayres, in April 1872, contained, according to Professor Kyle:

	Parts per Million.
Total solid matter.....	180 00
Chlorine.....	24.00
Free ammonia.....	0.07
Albumenoid ammonia..	0 34

Hardness, 4.5°.

In front of Belgrano, whence the new supply is to be drawn, the water contained on the 12th of April, 1872, according to the same authority:

	Parts per Million.
Total solid matter.....	130.00
Chlorine.....	17.00
Free ammonia.....	0.06
Albumenoid ammonia..	0.24

Hardness, 4.25°.

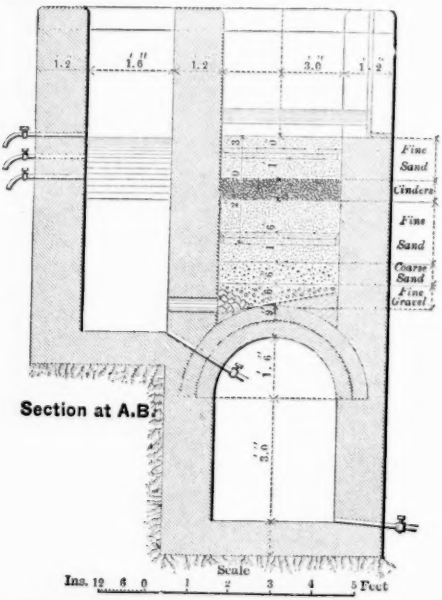
The quality of the water experimented on by the author varied almost daily, according to the changeable state of the river. The following, which is an analysis of a sample taken on the 3d of September, 1877, represents its average quality before filtration :

	Parts per Million.
Total solid residue.....	357.00
Chlorine.....	39.00
Free ammonia.....	0.026
Albumenoid ammonia...	0.230

Hardness, 6.4°.

This water, in its best condition, was dirty looking and yellowish. Left in a stoppered bottle, for months together, in perfect repose, it never became clear, always remaining yellowish and opalescent. The matter in suspension appeared to be finely comminuted clay, almost in a colloid or gluey form, and incapable of mechanical precipitation in any reasonable time. Only by the addition of alum or one of the persalts of iron did it seem possible to effect a precipitation of it. The finest filter paper, even in double or triple folds, was powerless to separate the impurity. The problem for solution was, to find some material that would effect a mechanical separation of the matter in suspension, as it was not considered advisable on many grounds to have recourse to chemical precipitation.

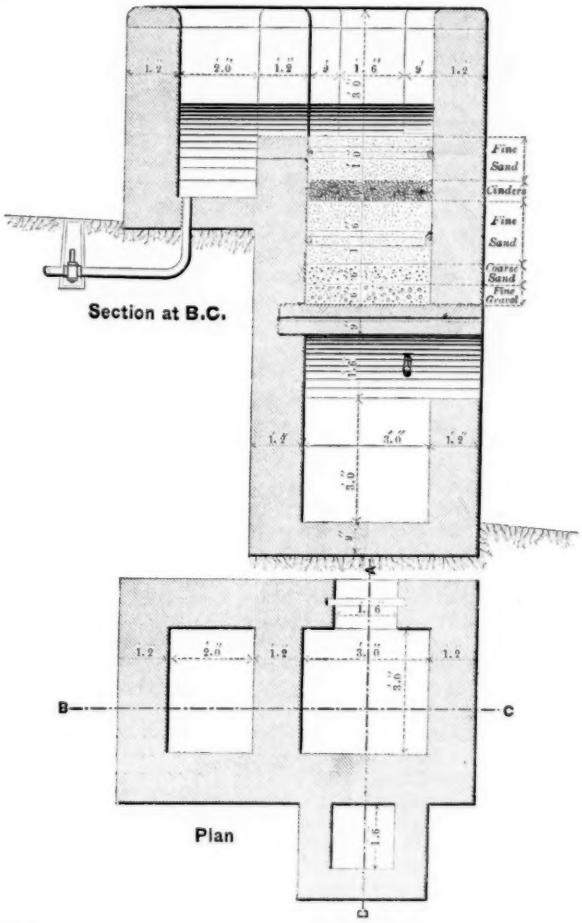
A small experimental filter was therefore constructed for the purpose (Figs. 1 and 2).



The area of the filter bed was one square yard; its depth, from the surface of the sand to the top of the arch, being four feet. Underneath the filter bed a chamber was constructed of the same area as the filter bed, and capable of containing one cubic yard of water. A small side chamber on the level of the filter bed, served for the admission of the water to be filtered. A pipe three inches in diameter was brought from the settling pools through the bottom of this chamber, and a valve on the pipe regulated the supply. The water, rising in the side chamber, flowed quietly over the surface of the filter without disturbing it. The height of the water was regulated by an opening in the side, which was provided with grooves to receive a stop plank of any desired height.

The water, after percolating the filtering material and reaching the bottom, passed through an opening into a small

chamber, eighteen inches square. At the upper part of this chamber a series of pipes and cocks was inserted, the highest pipe being at the same level as the surface of the filter, the others at intervals of three inches downwards. A pipe that could be attached to any of the cocks served to convey the water from this chamber to that for measuring underneath the filter. A discharge pipe from the bottom of the effluent chamber allowed the application of an unlimited head if desirable, and provided for the emptying of the filter; while another discharge pipe permitted the cleansing of the measuring chamber. The depth



of water in the measuring chamber was taken by a small graduated rod, introduced through the opening in front. The variations of head were obtained by opening or shutting the different cocks in the effluent chamber; or where a variation was required more minute than could be given by the cocks, it was effected by varying the height of the stop-plank in the filter chamber. This stop-plank, which served as an over-flow, ensured a regular height of water in the filter chamber.

The depth of water on the filter in the

present experiments in no case exceeded twelve inches, and was frequently less. The head given in the tables is the difference of level between the water in the filter chamber and the effluent water, which is the true head. In some treatises on water, confusion is created by speaking of the head of water on a filter as though it were the depth of water on the filter bed. The depth of water on the bed is of course one element in the question, but it has by itself no relative importance; it may be three inches or three feet without the head being in any way varied. By an unlimited head, as used in the tables, is meant that given by a free exit of the water from the bottom of the filter, when the only impediment to the discharge of the water is caused by the friction of the materials through which it passes.

The principal object sought at the commencement of the experiments was to ascertain the best material to use in the large filters then approaching completion, with a view to remove, if possible, the turbid yellow appearance from the water. For this purpose the disposition and quality of the materials were varied as the experiments proceeded. At the same time, experiments were carried on in the house, the instrument used being a small copper cylinder six inches in diameter and eighteen inches high, provided with a peforated plate at the bottom, and a stopcock, to which an india-rubber tube was applied. By varying the position of this tube any desirable head could be obtained.

The experiments on the large model filter showed some curious results as regarded the rate of filtration, which the author thinks worthy of attention. The materials first employed were those commonly used in England, viz: a layer of two feet of fine sand, nine inches of coarse sand, six inches of fine gravel, six inches of coarse gravel, and three inches of rubble. The rubble was packed by hand; the sand and gravel were gently pounded. The sand was brought from the coast of Uruguay, on the opposite side of the river to Buenos Ayres. It is a sharp clean sand, being everything that could be desired. The filter was started with the lower cock open, or say with an unlimited head. The discharge of water per square yard per day was a follows:

	Gallons.
1st day	9,323
2nd "	6,552
3rd "	3,309
4th "	1,524
5th "	220
6th "	39

The water that came from the filter was not satisfactory on any of these days. It was a little better on the sixth day, but was still quite turbid, and only slightly improved as regarded its other qualities. A deposit of fine mud, about $\frac{1}{16}$ inch in thickness, covered the top of the filter. This could be torn off like a piece of cloth, leaving the sand below perfectly clean. About $\frac{1}{4}$ inch of sand was removed from the filter, and it was started again with the same head. The discharge per square yard was then:

	Gallons.
1st day	2,994
2nd "	2,706
3rd "	1,707
4th "	60

Another $\frac{1}{4}$ inch of sand was now removed, and the filtration was resumed:

	Gallons.
1st day	2,081
2nd "	1,381
3rd "	1,334
4th "	1,278

The water during all this time continued of the same unsatisfactory character.

In order to reduce the speed the head was now altered to two feet, when the filter discharged:

	Gallons.
1st day	983
2nd "	1,108
3rd "	—
4th "	1,245
5th "	1,684
6th "	2,055

This rate being too great, the head was altered to one foot six inches, when the water ran:

	Gallons.
1st day	1,303
2nd "	2,185
3rd "	3,728

The filtered water was still very turbid and only slightly improved.

The extraordinary and rapid increase in the daily speed of filtration was so different to what had been expected, and

so inexplicable, that it was supposed some passage must have been opened through the sand. In order to satisfy himself on this point the author carefully removed the material, but found everything in order; the different layers had not intermingled, nor was there the slightest sign of any hole or passage through them.

The filter was, therefore, carefully remade, the same materials being used, and was started with a head of 2 feet, and gave the following result:

	Gallons.
1st day.....	1,059
2d "	2,348
3d "	3,729

The filter was then stopped for a day to put in some additional valves, and it was started again with a head of 12 inches. Under this head no water passed through, the water in the effluent chamber never rising to the necessary level. After leaving it for nearly a day in this position, the head was increased to 1 foot 6 inches, when the water immediately began to flow, and ran that day to the extent of 1,187 gallons; the next day the rate increased to 2,150 gallons; and as it showed the same tendency as before to a steady increase the head was changed to 1 foot, when it dropped to 711 gallons.

Thinking that the limited size of the filter, and the large proportionate area of the sides to that of the total area, might have some effect on the result obtained, by creating passages down the sides of the walls, the filter was emptied, and fillets of wood 1½ inch wide were firmly wedged up to the sides, at distances of 3 inches, and 2 feet from the surface of the sand. The upper parts or these fillets were weathered with pure cement, so as completely to prevent any flow of water down the sides.

In the former experiments the bottom cock had been left open when the filter was started, so that any air in the materials might be driven down by the water and pass out freely. Now, however, a 2-inch pipe was inserted in one angle, reaching down to the rubble, and the filter was then carefully remade, with the same material as before. It was now started with a head of 1 foot 6 inches. Its rate was, on the first day, 683 gallons; on the second day it rose to 1,602 gallons, when the head was altered to 1 foot; it then dropped to 817 gallons. The next day it was 1,479 gallons, and the following day 1,850. Without any apparent reason it then fell to 1,084 gallons, but rose on the next day to 1,400 gallons. From a scarcity of pressure in the settling pools, the head on the following day was only six inches, when the rate fell to 371 gallons. With the former head of one foot it rose again the succeeding day to 1,456 gallons; on the ninth day it was 1,220 gallons; on the tenth day, with eleven inches head, it was only 441 gallons; and on the eleventh day, with a 12-inch head, 734 gallons. The character of the filtered water during this time was still unsatisfactory, being yellowish and dirty; and from an analysis of the water filtered on August 4th, the qualities were otherwise found to be but slightly improved.

In the meanwhile, the experiments conducted in the house had led the author to the conclusion, that the object sought might be attained by the use of some other material in the filter bed. The topmost layer of sand was therefore taken out for a depth of one foot three inches, and replaced with a layer 4½ inches thick of picked and washed cinders, about the size of hazel nuts, below, with 10½ inches of fine sand on the top. With a head of six inches the filter then ran at the rate of 826 gallons for the first day, and 924 gallons for the second; but the quality of the water being still unsatisfactory, the top layer for two feet was removed, and (the lower portion of the filter remaining the same) was replaced with three inches of fine sand, one inch of coarse cinders, one foot two inches of pounded cinders (the fine dust having been washed away previously), and finally, as a top layer, six inches of fine sand. It was necessary to make the top layer of sand, as otherwise the cinders, being very light, floated up when water was admitted. The filter was then started with six inches head, and allowed the water to pass on the first day at a rate of 780 gallons, and on the second day 692 gallons. As the rate appeared to be diminishing rapidly the head was increased to nine inches, when the water percolated at the following rates:

	Gallons.
3rd day	1,156
4th "	1,335
5th "	1,502

The head was, therefore, altered back to six inches, when the flow per square yard was:

	Gallons.
5th day	881
6th "	880
7th "	827
8th "	784
9th "	763
10th "	643
11th "	468
12th "	391

This was the first occasion on which the filter had run approximately at the rate it was desired to establish, which was 700 gallons per square yard per twenty-four hours. The character of the water was now much superior. It had lost the yellow tinge; and although still of a milky look, it was considerably improved as regarded its other qualities.

The house experiments having shown that it was not desirable to remove the fine dust from the cinders, and that better results could be obtained by simply using the cinders as they came from the engines, the surface of the filter also commencing to show signs of dirt, it was determined to remake the filter, using in place of the pounded cinders those from the engine heaps, the coarser cinders being riddled out, so that none remained larger than a hazel nut. At the same time the author considered it desirable to ascertain whether there was any tendency in the different layers to run together, and therefore carefully examined them one by one as the filtering materials were removed. The examination showed that there was no tendency to run together; and as it was desirable to get a great depth of upper material for the purposes of varying them as much as possible, the lower part of the filter was altered as follows: the portion of the filter round the exit-hole was packed with small rubble, and a layer of fine gravel, about the size of peas or horse-beans, was laid on for a depth of six inches. On this was placed a layer of coarse sand for six inches, then one foot of fine sand, then one foot of finely-sifted cinders (not powdered), and on the top of all one foot of fine sand. The cinders in this case were taken from the heap outside the engine house, being riddled so as to remove all pieces above the size of a hazel nut.

The filter was started under a head of six inches, and the water percolated at the daily rate of 1,201 gallons per square yard. As this rate was greater than eesirable, the filter was stopped, and two additional effluent cocks were put in at a higher level. At the same time the top layers were altered to one foot three inches of fine sifted cinders, similar to the former ones, covered with nine inches of fine sand. The filter was started with three inches of head; the flow of water per square yard was as follows:

	Gallon.
1st day	248
2nd "	348
3rd "	450
4th "	433
5th "	505
6th "	467
7th "	434
8th "	434
9th "	333

The quality of the water was good. It was almost clear and transparent, and had entirely lost its opalescent character.

The surface of the sand was now scraped, and as the rate was too small, the head was increased to five inches, with the following results;

	Gallons.
1st day	611
2d "	890
3d "	1,201
4th "	1,099
5th "	868
6th "	359
7th "	191

The water still remained slightly milky in appearance. In order to remove this the filtering materials were altered in the manner shown to be advisable by the house experiments. Instead of using the cinders and ashes just as they came from the heap, the heaps were riddled through a coarse sieve which retained only the cinders above the size of a walnut. These were washed, and afterwards well pounded into a fine black dust. The lower part of the filter remaining the same, the upper layer of three feet was then taken out, and replaced with fine sand, twelve inches; pounded cinders, six inches; fine sand on top, eighteen inches; total, thirty-six inches. The cinders were moistened before

being put in, to prevent the dust blowing about, and were punned in rather tight. The filter was started with a head of one foot ten inches, when it yielded:

	Gallons.
1st day	240
2d "	168

The bottom valve was then opened, and the head made unlimited, when it supplied:

	Gallons.
1st day	352
2d "	354

The water on these four days was excellent. It was brilliantly clear and bright, all trace of milkiness having vanished, and analysis showed that its other qualities were equally improved. The speed, however, was too small, resulting from the too close punning of the cinders; the materials were therefore taken out, and replaced as follows: fine sand, eighteen inches; pounded cinders, six inches; fine sand (top), twelve inches: total, thirty-six inches. The materials were gently pounded in layers. The filter was started with a head of four inches of water, and yielded per square yard:

	Gallons.
1st day	231
2d "	175
3d "	200

On the head being altered to 10 inches, the rate of flow became:

	Gallons.
3rd day	636
4th "	601
5th "	768
6th "	997
7th "	991
8th "	1,100
9th "	1,141
10th "	1,137

The steady increase in the rate of speed with an unvarying head will be again noticed. The quality of the water, however, did not seem to be affected, but continued clear and transparent to the last.

On the following day the settling pools were drawn off for cleansing, and circumstances prevented the resumption of the experiments. This is much to be regretted, as the results now submitted can only be considered as fragmentary and incomplete.

It is evident that sand alone will not remove the turbid appearance from the water; but it would be desirable to continue an extensive series of experiments on the sand filter as first constructed, to procure precise data as to head of water requisite for different speeds of filtration, as well as to ascertain what results could be obtained in the way of purifying the water. Time did not permit of this then, as it was desired to ascertain, as soon as possible, what material would clear the water. It was intended afterwards to resume experiments on the sand filter; but this plan shared the fate of many other good intentions. Again, as regards the position of the cinder bed and the varying thicknesses of sand, no decided opinion can be given, inasmuch as the experiments never got beyond their first stage. Such as the experiments are, however, perhaps some general opinions may be deduced from them which it is probable further and more complete experiments would confirm.

It has been generally regarded as an axiom that, to procure efficient filtration, the speed at which the water passes the filter should not exceed 12 cubic feet per square foot of area of filter per twenty-four hours, or say 675 gallons per square yard per day. As regards this rate, if it can be preserved, the results obtained from these experiments justify the supposition that it is an effective one, and that it should not be much increased. The head necessary to produce this rate through an ordinary sand filter, such as is used in England, with a depth of material of from 4 to 5 feet, has been variously fixed by engineers at from 1 foot 6 inches to 2 feet. The maximum head allowed by Mr. Bateman is, the Author believes, 1 foot, the minimum being about 6 inches, the head varying in proportion as the sand becomes clogged and the porosity of the filter decreases. The experiments now recorded lead to the belief that even a head of 1 foot would be too great for such a filter, and that the proper head would vary between 4 inches and 6 inches. They also show that any calculations as to the average rate of filtration are fallacious. It appears almost impossible to preserve a uniform speed, which may vary with trivial and unavoidable circumstances to as much as double that wished for. The most trifling alteration of head seemed to alter the speed of filtration in a manner quite unexpected. Under an unvary-

ing head constant variations would take place in the speed, while the well-marked tendency to a daily increase of discharge, when the conditions as to head and quality of the water are unaltered, is not the least curious part of the phenomena. Slight differences, also, in the mode of packing the materials have a marked effect on the discharge of the filter.

It would appear possible to effect some reduction in the thickness of materials composing the bottom of the filter. In the first construction, sand occupied the upper 2 feet of the filter, and beds of sand and gravel, gradually increasing in size downwards, the remaining or lower 2 feet. In the later construction of the filter these lower layers were replaced by two, each 6 inches in thickness, the lowest being of fine gravel, the upper of coarse sand. The results given by this latter construction as regards keeping up of the sand, were quite as satisfactory as those derived from the four layers, 2 feet in thickness, employed in the first instance, and it is believed that even the thickness of 1 foot might be further reduced.

In a sand filter, the sand being the real filtering material, it is desirable to have as great a depth of it as possible, not only to secure greater depth of sand and greater thickness to scrape from, but also to ensure a more uniform rate of filtration. The more homogeneous the materials of which a filter bed is composed, the greater chance will there be of maintaining a uniform rate of speed.

As regards the materials of which the filtering layers were composed, the variations introduced into these were suggested by the result of experiments carried on in the house. Numerous materials were experimented on, with unsatisfactory results in all cases except in the present one.

The Author was led finally to the use of cinders in the search after some material that should have extremely fine angles and points, and partly from his recollection of their usefulness in stopping leaks in canal banks. In the satisfactory results ultimately obtained, the ordinary cinders from the engine fires were riddled out from the dust and finer pieces; they were then well washed, and crushed to powder. At first the fine dust was separated either by winnowing it or by throwing the cinders into water and allowing the dust which rose to the top to flow away; but experience showed that the cinders so treated did not give a satisfactory result, the effluent water still remaining milky looking. To secure a perfectly brilliant pellucid water, it was necessary to pound the cinders very fine and not separate the dust.

It will be seen that in every case there was a marked increase in the hardness of the water after filtration. Looking at this, and knowing that certain salts had the power of precipitating the suspended matter, the idea naturally occurred that the clearness of the water was obtained from a chemical action set up by some element in the cinders, which would probably disappear in time. This does not, however, appear to be the case, inasmuch as no thickness of cinders cleared the water when they were not sufficiently fine or the dust had been removed, whilst a thickness of only 2 inches effectually did so when the cinders were finely pounded. Had the action been a chemical one the effect would, it was suggested, have been the same in both cases. It may be said that the pounding fine of the cinders allowed them a freer action on the water; but at one time a thickness of 1 foot 3 inches of fine cinders was used, and any extra fineness in the smaller 2-inch layer might be supposed to be more than compensated for by the great extra thickness of the coarser layers. The Author, therefore, was led to the conclusion that the action was purely mechanical, and that the clearing of the water was effected by the detention of the fine particles of mud by the needlelike filaments and points of the cinder powder through which they had to force their way. If this is so, it is natural to conclude that in course of time the filter would become clogged; but from the experiments in the house it seems as if this did not take place, at any rate for a long period of time, It would appear as if the amount of suspended matter escaping the sand, sufficient to give an opalescent or yellowish character to the water, is so excessively minute, that some time would elapse before any effect would be produced on the cinders. The principal part of the suspended matter is deposited on the top of

the sand; when the surface is scraped the eye cannot detect any discoloration in the layer of sand below.

In the filter shown in Fig. 2, the thickness of the cinder bed is 6 inches, the thickness of sand above it being 1 foot, and below it one foot 6 inches. The lower layers are composed of 6 inches of coarse sand and 6 inches of fine gravel. This arrangement is not put forward as a sample of how the filter should be made, but simply as showing how it happened to be constructed at one particular stage of the experiments, when they were brought to a sudden conclusion. The construction of the filter rendered it necessary that the surface of the filter should be 4 feet above the bottom, and it was essential in any case to fill up the lower portion. No conclusions, therefore, should be drawn from this particular form. Experiments, continued for some time longer in the house, showed that excellent results could be obtained with a total depth of filtering matter of 1 foot, the cinder bed being 2 inches thick.

If it was desired to keep down the depth of the filters, an effective filter could probably be made with 6 inches of fine clean gravel, 2 inches of coarse sand, 6 inches of fine sand, 6 inches of cinders, and 6 inches of fine sand on the top. The thickness of sand on the top might be advantageously increased, so as to provide surface for scraping, without the necessity of disturbing the lower layers for some time. Experiments would be required to show how far such a construction as that recommended would be advisable. The filter shown in Fig. 2 was a success, and there can be little doubt that the whole effect of that filter was obtained in the upper 1 foot 6 inches, the remaining 2 feet 6 inches being useless as regarded filtration.

During the progress of the experiments frequent analyses were made of the water, both before it passed through the filter and afterwards, with the object of ascertaining the effect of the filter on the water. The system of analysis employed by the Author was that recommended by Professor Wanklyn, viz., the determination of the total solids in the usual manner; of the chlorine by means of a titrated solution of nitrate of silver. Free ammonia and of the albumenoid ammonia, produced by destructive distillation of the residue, were estimated by Nessler's process. Dr. Anderson has shown that this system cannot be depended on as a means of ascertaining the nitrogenous matter in water, for much of this matter has a greater tendency to combine with oxygen to form nitro-oxides than with hydrogen to form ammonia; consequently much of the nitrogenous matter frequently remains in the retort and is lost sight of, in the determination of albumenoid ammonia in the way proposed by Professor Wanklyn. In the present case, as only a comparative analysis was required, the system adopted may be considered sufficiently accurate.

The Author proposes to cite a few out of numerous analyses, as sufficient to prove the position he wishes to put forward. As regarded the sand filter, the water that passed through in the earlier experiments was little improved in quality, though slightly in appearance. Not until the head of water had been reduced to 1 foot, and a more regular rate of flow established, was any marked improvement effected.

On the 4th of August the analysis of a sample of water, taken from above the experimental filter, gave

	Parts per Million.
Total solid residue	342.00
Chlorine	42.00
Free ammonia	0.005
Albumenoid ammonia	0.24
Hardness 6°.	

Water was then passing through the filter at the rate of 1,084 gallons per square yard of surface per day. A sample of water taken from beneath the filter on the same day gave

	Parts per Million.
Total solid residue	314.00
Chlorine	42.00
Free ammonia	0.008
Albumenoid	0.194
Hardness 6°.	

This water was a good deal improved in outward appearance, but was still opalescent and yellowish. A slight improvement only had been effected in its character.

A sample of water from above the filter, on the 5th of September, gave on analysis:

	Parts per Million.
Total solid residue	357.00
Chlorine	39.00
Free ammonia	0.026
Albumenoid ammonia	0.23

Hardness 6°.4.

A sample of water from below, on the same day, contained:

	Parts per Million.
Total solid residue	357.00
Chlorine	39.00
Free ammonia	0.013
Albumenoid ammonia	0.070

The state of the unfiltered water at this date was very good. It contained comparatively little sediment, but had the characteristic opalescent yellowish look peculiar to rivers of the class of the Plate. The filtered specimen was almost clear. It had entirely lost the characteristic yellow tinge, and merely retained a slight milky look. It was, as the analysis shows, considerably improved in quality.

The sample taken from below the filter on September 26th gave:

	Parts per Million.
Total solid residue	285.00
Chlorine	39.00
Free ammonia	0.018
Albumenoid ammonia	0.05

Hardness 7°.

The rate of filtration when this sample was taken was 334 gallons per square yard per day. The water was crystalline, every trace of yellow having vanished.

The two following samples were filtered through a layer of six inches of cinders and of six inches of sand, a layer of about one inch of sand being spread over the cinders to keep them in their place. The specimens were filtered in the house through the small copper filter, the speed being at the rate of about 600 to 700 gallons per square yard per day.

	Aug. 10th. Parts per Million.	Sept. 12th. Parts per Million.
Total solid residue	214.00	242.00
Chlorine	42.00	39.00
Free ammonia	Trace	0.00
Albumenoid ammonia	0.03	0.01
	Hardness 10°.	Hardness 11°.

The water used for the purpose of obtaining these samples was in its usual condition. It may be taken as containing—

	Parts per Million.
Total solid residue	250.00
Chlorine	40.00
Free ammonia	0.05
Albumenoid ammonia	0.24

Hardness 6°. 5.

Both filtered specimens were clear and crystalline.

Samples taken in October from beneath the experimental filter, when composed as shown in Fig. 2, gave equally favorable results.

Unless the water came away bright and clear the results, as regarded the albumenoid ammonia, were not satisfactory. In London the water companies, starting with a water containing 0.24 part per million of albumenoid ammonia are said to obtain a product containing only 0.07 part, more or less ; or in round numbers, they reduce the amount of albumenoid ammonia to one-third by the simple use of a sand filter. The Author never obtained such results. No filtration, however slowly or carefully performed, through sand alone, would remove more than one-half of the albumenoid ammonia. It was not until he succeeded in removing the color entirely from the water that he also freed it from the albumenoid ammonia. It would almost appear as though there were, in the case of the River Plate, some connection between the amount of albumenoid ammonia and the turbid yellowish appearance of the water. In almost every case where the desired result was obtained, there was a marked increase in the hardness of the water.

It is much to be regretted that these experiments were so fragmentary and incomplete. So far they indicate that the problem of purifying delta waters, such as those of the rivers Mississippi, Hooghly, and Plate, so as to render them clear and sparkling, is not difficult, and that it can be done a large scale without expense than that of ordinary filtration. Additional experiments would be desirable to determine the best position of the various layers composing the filter and their minimum thickness, when probably further interesting details might be obtained.

THE ICE-BOAT PROBLEM.

By Brvt. Maj.-Gen. Z. B. TOWER, Corps of Engineers, U. S. A.

In my paper on the Ice-Boat, published in the December number of this magazine, I assumed that the wind at right angles to the sail, giving the greatest propelling power, would also give the boat its maximum speed. This would be true if friction were the only resistance to motion. The air's resistance, however, complicates the conditions of the problem. General equations No. 5 in my last paper, introduced this element as a definite function of the variables and constants. These equations were wrought out at the moment of the magazine's going to press, and were not examined by me at the time.

They are as follows:

$$\left\{\begin{array}{l} \tan x=[(f+f'v^d)\sqrt{1+c^2} \\ \qquad +f''c(v+w\cos(u+x)^2] \\ \text{C.S.}(w\sin u-v\sin x)^2\sin x=\tan x\dfrac{W}{c} \end{array}\right.$$

$$(6)\quad \text{C.S. } w^2(\sin u-y\sin x)^2\sin x=\tan x\frac{W}{c}$$

When angle $(u+x)$ is greater than 90° its cosine becomes negative and is $=-\cos(u-x)$, the angle u being reckoned from the stern of the boat forward, instead of from the bow aft as in the original equations (5). As this angle u diminishes from 90°, the expression $-\cos(u-x)$ decreases more rapidly than $\sin u$ in the first member of the equation. Hence the resistance diminishing more rapidly up to a certain limit than the propelling power, the maximum speed will result from angle u less than 90°.

Equation (6) can be solved with reference to y and, by differentiation, an equation obtained showing the relations between angles u and x and factor c and the constants, when y is a maximum. But this resulting equation is too complicated for use.

The greatest value of y may be found, approximately, by the process of successive substitutions of different values for the three variables.

Equations (5) may be written thus, substituting

$$-\cos(u-x) \text{ for } +\cos(u+x).$$

$$(7)\quad y=\frac{1}{\sqrt{c}}\sqrt{\frac{\tan x-(f+f'v^d)\sqrt{1+c^2}}{f''w^2}}+\cos(u-x)$$

$$(8)\quad y=\frac{\sin u}{\sin x}-\frac{1}{\sqrt{c}}\times\sqrt{\frac{W}{\text{C.S.}w^2}}\frac{}{\sin x\cos x^{\frac{1}{2}}}$$

Let C be as before .008, S the sail's surface=400 square feet, W, weight of boat, 800 lbs.

f'' as found is equal to $\frac{C'F}{W}$, C' being .005 lbs. the unit of pressure for small surfaces, and F number of square feet, which the boat exposes to the resistance of the air, including the helmsman.

Estimate of exposed surface is as follows:

Vertical section of mast (27 ft. by an average of 4 in)....=9	ft.
Being cylindrical its equivalent is $\frac{3}{4}$ 9 ft.....=6.75	"
Helmsman (side surface exposed to wind).....=5	"
Front or bow beveled $20'\times 2\frac{1}{2}''$ $=4\frac{1}{6}$ ft.$\times.7$.....=2.92	"
Support of mast and iron rods, say.....=1.33	"
Total.....16.00	sq.ft

Substituting the above constants in eqs. (7) and (8) there results

$(f+f'v^d)$ being assumed$=\frac{1}{30}$

$$(7)\quad y=\frac{1}{\sqrt{c}}\sqrt{\frac{\tan x}{.09}-\frac{1}{2.7}\sqrt{1+c^2}}+\cos(u-x)$$

$$(8)\quad y=\frac{\sin u}{\sin x}-\frac{.52705}{\sin x\cos x^{\frac{1}{2}}}\times\frac{1}{\sqrt{c}}$$

From several successive substitutions of different values of the variables, I find the maximum of y to be, approximately, 1.84, corresponding to

angle $x=17°$

and angle $u=74°$, to $75°$; or measured from the bow aft, with direction of boat, 124°, to 123° C being 1.6.

The friction on the ice is 31 lbs., and the air's resistance, 122 lbs.

Total resistance to boats motion 153 lbs.

These substitutions, however, have not been sufficiently extended to obtain the exact angles. In fact the variations, near the maximum of y, are so small, that a change of one or two degrees in u affects it but slightly. Each is probably within one degree of the true angle.

C', the unit of pressure of the wind upon small surfaces, taken at .005 lbs., may be too large. It might be more correct to assume it .004, or even less, as the surfaces exposed to the air's resistance, making up the 16 square feet, are very narrow and do not hold wind.

With a sail that spreads 500 square feet, and C' assumed =.004, y would doubtless exceed 2; that is, the boat would reach a maximum speed a little greater than twice that of the wind.

The value of the factor $c=1.6$ shows that the boat, sailing under the conditions assumed, has an excess of stability. For, if the height of the sail's center of figure is ten feet above the plane of the ice, and the boat twenty feet wide, the ratio of the moment of the boat's weight, to that of the pressure of the sail at right angles to the boat's direction, will be $\frac{16}{10}$.

It is an interesting problem to determine the values of angles u and x, and, incidentally, of the factor c, to enable the boat to work to the windward most rapidly.

That condition will be expressed by

$$\left.\begin{matrix} v\cos(u+x) \\ \text{or,}\quad y\cos(u+x)\end{matrix}\right\} \text{a maximum.}$$

As the most recently constructed ice boats are made of one longitudinal and one cross plank, braced diagonally by iron rods, the canvas cover suggested in my last paper would evidently be inappropriate.

NOTE—On page 15, column 1, December number of *Magazine*, read, "The value of y in eq. C. S. $w^2(1-y\sin x)^2\sin x=0$, is

$$y=\frac{1}{\sin x}$$

instead of what is printed between the 17th and 24th lines.

REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA.—At the meeting of the Club held November 15th, 1879, nineteen members were present.

Mr. Percival Roberts, Jr., read "Notes Upon a Recent Decision of the U. S. Circuit Court, in the suit of Atkins Bros. *vs.* Edgemoor Iron Co., in this City."

The decision was in a suit brought to recover damages upon a lot of structural iron furnished by plaintiffs to defendant, who refused to settle the original account by an amount equal to expenses incurred in rendering said material suitable for intended use. Judge McKennan's charge to the jury cannot fail to be of great interest and importance, not only to engineers and manufacturers, but to all parties connected with transactions in constructive materials. It was the object of the paper not to criticise the rulings of the Court, but to make a few suggestions in regard to results arising from this decision, which will be cited in future, no doubt, as of much importance in legal disputes.

In this case there was no written contract; merely a verbal understanding and statement that the iron to be furnished was for special purposes.

The Judge, in his charge, said:

"That although there was no express agreement, still, under all the circumstances, there was an implied obligation on the part of the plaintiffs to furnish such iron as was suitable to be used in these structures * * * The law implies a warrant that the iron furnished under contract shall be adapted in quality and otherwise to intended use: and that if, at any time afterwards, it is ascertained and is satisfactorily shown that the iron was not of such quality as was fairly adaptable to the use for which it was intended, the warrant was broken, and defendants are entitled to damages."

The question naturally arises under such a ruling, when does a guarantee end?

Who shall be the judge of the fitness of material; what the criterion of its quality? It may be answered, the testimony of experts must be employed. But where are two experts who agree exactly as to the necessary qualifications for structural material?

Mr. Roberts further urged the importance of full and practical specifications being prepared for all work, and being transmitted not only to the builder, but also to the manufacturer. The questions brought out in this paper are also a strong proof of the urgent need, existing in this country, for thorough and systematic investigation of the strength of materials. This is work, the expense of which cannot be borne by individuals or corporations — the Government alone can bear so great a burden, but a burden made light by the vast importance of consequent results.

A Note upon "The Connecting Rod," by Prof. William D. Marks, was read by the secretary.

Mr. Billin read some notes upon the "Preservation of Timber." The opinions of many of our principal road-masters, in regard to the life of ties, seem to be very diverse. They, however, place the life of a white oak tie, cut

when the sap is down, seasoned and laid in good gravel ballast, at between seven and ten years.

The majority of road-masters apparently live in blissful ignorance of the beneficial effects derived from the use of preservatives; and it is undoubtedly on account of such ignorance that more decided steps have not been taken in this country toward economizing our timber supply, and the cost of maintenance of way of our railroads, by increasing the life of timber used.

Some few experiments have been made within the last ten years toward strengthening the life of ties by Burnettizing, etc.; but the processes were applied in a very crude and imperfect way, and the partial failure of these experiments has done much toward preventing the introduction, now, of new and thorough processes.

It is estimated that seven million acres of timber are cut each year, in order to furnish ties for railroad use. These figures are not an exaggeration, and it is only astonishing that their magnitude has not, before the past year or two, attracted more attention to the subject and led to the adoption of some preservative process by our larger railroad companies.

Instances were cited of English creosoted ties which had been in use for twenty and twenty-two years, and were in as good state of preservation as when put in track. Creosoted piles driven at Portsmouth, England, forty-two years ago, were stated to have been found as good above water-line as below, and to have outlived sixteen and seventeen sets of piles cut from the same timber and driven in the same work, but which were not creosoted.

A further discussion of "Myers' Formula for Proportioning Culverts" was taken part in by Messrs. Herring, Darrach, and Cleemann. This subject has received much attention from several members of the Club; but an understanding of it cannot be had until the papers and various discussions relating to it are published in full in the proceedings of the Club.

Mr. J. E. Codman made some remarks upon the "Butler Mine Fire cut-off," and read a letter from Mr. C. J. Conrad in regard to it. The portion of this work where the danger lay was in the tunneled part, where it was feared that heating of the rock would carry destruction over the archway and communicate the fire to the seams in the workings of the Pennsylvania Coal Company; and, once there, no power on earth could have prevented it from working its way under the town of Pittston. All work at the fire was finished September 30th, and changes occurring since then have only served to confirm the announcement then made, that the Cut-off was a complete success. There was no question about the success of any portion of the work except the tunnel. The walls of this were built "dry," from eighteen to thirty-two feet thick, and eighteen feet high. The wall on the fire side was heated to a *white heat* through to the exposed face, but cooled-off in a few weeks. Finally, the great heat penetrated fifty feet of rock and earth, weakening, and disintegrating the mass, so that it crumbled and fell, filling the tunnel-space with broken rock; but this did not occur until the fire had spent itself and the walls were all cool.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The last number of the Transactions contains an interesting paper on an important topic, viz.: Interoceanic Canal Projects, by A. G. Menocal.

After considering the various plans and routes, Mr. Menocal draws the following conclusions:

From the above statements and considerations it seems to follow:—

1st. That however desirable a canal at the level of the sea, partaking of the nature of a strait may be, to better satisfy the demands of trade, its execution, either with or without a tunnel, presents so many difficulties and doubtful elements as to place its probable cost out of the range of a successful commercial enterprise.

2d. That a canal with locks can be so constructed as to satisfy all the requirements of ocean navigation, at a cost within the possibility of a private undertaking, with reasonable expectations of liberal returns and without overtaxing the commerce of the world intended to be benefited thereby.

3d. That while a canal with locks seems to be practicable, *via* both Panama and Nicaraugua, the latter route possesses greater facilities for the execution of the work at a reduced estimate of cost based on sufficient information to eliminate unknown elements, which might materially so alter the conditions of the project, as to cause painful disappointment to take the place of long-deferred hopes and cheering expectations.

And furthermore, that the geographical position of Nicaraugua is more favorable to the United States, whose commerce will contribute more than that of any other nation to the business of the canal, while it will afford as great commercial advantages to foreign nations as other routes more to the south.

IRON AND STEEL NOTES.

KRUPP'S HOMOGENEOUS IRON.—After a long series of successful experiments Krupp has placed his "Fluss-Eisen" upon the market as a substitute for malleable iron. With a content of about one-tenth of one per cent. of carbon it resembles the best forged iron, but is superior to it. In large forged pieces it has a resistance of from 38 to 42 kilogrammes per square millimeter (54,046.7 to 59,735.8 lbs. per square inch). In sheet iron the tenacity is raised to between 40 and 48 kilogrammes (56,891.3 to 68,269.5 lbs. per square inch), with an elongation of about 25 per cent. and a diminution of 50 per cent. in the section of the experimental bar at the moment of rupture. The price is little higher than that of ordinary iron. The material is specially valuable for shafts, stern-posts, anchors, machinery, and all work which requires welding when it is made of common iron.—*Ann. du Génie Civil.*

A new industry for our iron mills, in which New England has succeeded tolerably well, but which has been made a specialty of in Eastern Pennsylvania, is the building of portable railroads for export to the West Indies. They greatly reduce the expenses of harvesting sugar cane, enabling the planters to transport heavy loads of cane quickly and cheaply from the distant fields to the sugar mills. The rolling stock used consists of light, four-wheeled platform cars, weighing less than a ton, which are capable of carrying over a ton of load. They are usually hauled by animals, but lately locomotives of very light pattern have been introduced. A portion of the plantation road often consists of a fixed track, from which branches of portable tracks are carried to the middle of the cane fields.—*Commercial Bulletin.*

RAILWAY NOTES.

An improved system of tramways was described in a paper read before the members of the Manchester Scientific and Mechanical Society at their last meeting, by Mr. W. Telford Gunson, C.E. The new system consists in laying down a continuous length of stone sleepers similar to the kerbs now in use for footpaths. In these kerbs longitudinal grooves are cut $1\frac{1}{4}$ inches deep, $3\frac{1}{4}$ inches wide at the top, and $3\frac{1}{8}$ inches wide at the bottom; the bottom of the groove is then covered with a $\frac{1}{4}$ inch layer of rock asphalte, and on this the rails are laid, the joints between the sides of the rail and the sides of the groove being filled in with a fusible mineral cement, thus firmly imbedding them in the sleeper, in addition to which they can, if desired, be further fastened down in the ordinary way. At the junction of the rails a quarter-inch iron plate, eight inches long, is inserted under the joint and lead run in, thus making the rails perfectly immovable. Mr. Gunson claimed for his system that it avoided all the well-known disadvantages of those at present in use, that vehicles of all descriptions could use it, and that whilst the cost of laying down was much the same, the cost of maintenance was considerably less than in other systems. The paper met with some criticism, and the discussion, in which it was urged that no tramway sytem would be perfect unless it was available for all classes of vehicles, was adjourned to the next meeting.

ENGINEERING STRUCTURES.

The Bridge of Lessart.—The Western Railway Company of France are at present engaged in constructing a branch line from Dol to Dinan; this has to cross the river Rance about two miles below the latter town by a single span at an elevation of about 100 feet. The method by which this has been accomplished is considered unique in this country, and deserves some notice. The iron portion of the bridge itself is of the usual diagonal construction; its length is 96.5 meters, or 314 feet, and its total weight 1,300,000 kilogrammes, equal to about 1,250 tons. It has been constructed by the Maison Jolly, of Argenteuil, a well-known firm, and has by them been put together on the line of railway immediately joining the intended span. Large wheels or rollers were placed beneath the bridge, and it was determined to push or propel the bridge across the stream by hydraulic power. In order, however, to regulate this operation, and to prevent its over-topping into the river below, a portion of a viaduct of a lighter construction, intended for another section of the line, has been temporarily attached or bolted to the fore end of the main span; this is about 150 feet in length, while another section is attached to the near end of the main iron bridge. It is evident that by adopting this method the fore end of the total iron construction will arrive at the supporting pier on the further side before the center of gravity of the main span will have passed that on the home side; it will then be supported at both ends until finally placed in position. As soon as this is accomplished, the two sections of the lighter viaduct will be removed. To M. Morse, engineer-in-chief to the railway company, is due the credit for this bold and original conception, and under his able direction and supervision this great undertaking has just been successfully accomplished.

It has been reported from the spot that on the northern side of the Great St. Gothard, at Goeschenen, the works reached the exact middle of the tunnel at fifteen minutes before nine in the morning of October 31st. The distance was 7,460 meters, or four miles and two-thirds of a mile.

ORDNANCE AND NAVAL.

A Floating Fighting Island.—Something more than rumor asserts that Messrs. John Elder & Co. have received an order from the Russian Government for the construction of an armor-plated war vessel, which is to be 500 feet long, to have a displacement of 17,000 tons, and to be propelled by engines indicating 10,000-horse power. With an average draught of say 22 feet, such a craft must have a beam of not much less than 75 feet to get the stated displacement. She will resemble an ordinary ship in very few respects. Presenting no side to the sea, her upper deck will be flatly curved, rising from the water's edge to the middle like the upper shell of a tortoise. She will, in a word, resemble a floating island upon whose sloping beach the waves will wash, rather than a ship. What her armament will be we cannot say, for, as may well be understood, the utmost reticence is for the present observed concerning this novel craft. She may no doubt mount six 100 ton guns, or perhaps eight, and may be fitted with a complete torpedo armament as well. Work is much wanted on the Clyde; and the construction of such a ship would afford employment for many months to come to hundreds of hands. It is but fit that the order should go to Messrs. J. Elder & Co., for it will be remembered that the late John Elder was one of the first, if not the first, to propose the construction of circular ironclads; and the ship concerning which we write will have much

in common with his views. In May, 1868, he delivered a lecture to the United Service Institution, in which he proposed the construction of a ship of war, circular, propelled by a system of submerged propellers, and with a bottom of the form of a segment of a sphere. She was to be protected by a belt of armor and a deck of great strength, on which was to be mounted a turret suitably armed with guns. Whether a similar idea occurred to Admiral Popoff, independently of all knowledge of Mr. Elder's schemes or not, we shall not pretend to say. As is well known, the two circular ironclads which the Russians now possess are of little, if any, value for warlike purposes. The new ship is intended, no doubt, to act as a counterpoise in the balance of power to the great ironclads of Italy, and to our own Inflexible. For the moment, it would appear that the union of Germany and Austria leaves Russia isolated. The fact seems, however, likely to act rather as a stimulant to her warlike propensities than the reverse. It is well understood in certain circles that important fortifications, of which the world hears very little, are springing into existence on the shores of the Black Sea and elsewhere. Forts defended by Herr Gruson's chilled cast iron plates, some 2 feet thick, may play an important part in any future war.

BREECH-LOADING ORDNANCE.—A decisive step has just been taken by the War-office in reference to the breech-loading question. Orders have been given for the manufacture of two large breech-loading guns at the Royal Arsenal, one to weigh 25 tons and the other 40 tons. It is expected that these new and necessarily experimental guns will be completed by the middle of next year, though possibly a little latter. The precise method to be adopted as concerns the breech-closing arrangement is not yet disclosed; but it is certain that the guns themselves will be constructed on the Fraser system. Despite all attempts made to disparage that system, especially by the advocates of steel, it holds its own, and appears likely to do so. The forthcoming breech-loaders will have a thicker steel core than is customary with muzzle-loading guns of the same caliber, but that is simply due to the fact that greater thickness of steel is required in order to provide for the breech-closing arrangement. An important element in the new guns will consist in the unusual length of the bore, this elongation being designed to utilize the force of slow-burning powder. It is evident that breech-loading has recently risen in favor with the authorities, owing to the facilities which it affords for working guns of extra length. It is understood that the designs for these guns were prepared a year ago, and therefore long before the recent Krupp demonstration at Meppen. But the preparation of the designs was apparently subsequent to the experiments with Krupp guns at Bredelar and Meppen, in the months of June and July last year, at which trials General Younghusband, Superintendent of the Royal Gun Factories at Woolwich, was present, accompanied by Captain Morley, in accordance with instructions from General Campbell, the Director of Artillery.—*Engineer.*

BOOK NOTICES

MATHEMATICAL TABLES, CHIEFLY TO FOUR FIGURES. By PROF. JAMES MILLS PIERCE. Boston; Ginn & Heath.

The tables are—A table of Logarithms—A table of Logarithms of Sums and Differences—Logarithms of Circular Functions—Inverse Circular Functions—Logarithms of Hyperbolic Functions—Natural Sines and Cosines—Natural Tangents and Co-tangents—Natural Secants and Co-secants—Proportional Parts.

The explanations of the tables are fully given, but the entire book is only a thin octavo of less than fifty pages. The tables are clearly printed on heavy paper.

FUEL: ITS COMBUSTION AND ECONOMY. Consisting of Abridgments of "Treatise on the Combustion of Coal, and the Prevention of Smoke." By C. WYE WILLIAMS, A.I.C.E., and "The Economy of Fuel," by T. SYMES PRIDEAUX. With Extensive Additions on Recent Practice in the Combustion and Economy of Fuel: Coal, Coke, Wood, Petroleum, Etc., by the Editor, DR. KINNEAR CLARK, C.E., Member of the Institution of Civil Engineers, Author of a "Manual of Rules, Tables and Data," "Tramways; their Construction and Working," Etc., Etc. London: Crosby, Lockwood & Co. New York: D. Van Nostrand, 23 Murray and 27 Warren Streets, 1879.

An important and timely book on a most important subject. The names of the authors whose treatises form the basis of the work are alone a sufficient guarantee of the value of the book. Most of those who think there is nothing left to learn in their practice of fuel consumption, will probably rise from a perusal of this work with surprise at their enlarged views of the real extent of the subject. The book should be in the hands of every owner of a steam-boiler, furnace, or other appliance for applying heat to industrial purposes. It is neatly bound in cloth, comprises 394 pages, with nemerous engravings and copious index, and its retail price is only $1.50.—*Scientific News.*

GIRDER MAKING AND THE PRACTICE OF BRIDGE BUILDING IN WROUGHT IRON. By EDWARD HUTCHINSON, M.I.M.E. London: E. & F. N. Spon. 1879.

In designing girders and bridges there are questions of an entirely practical character which it is necessary, with a view to economy, to take into consideration, as it is to apportion sectional areas to the calculated strains on different parts. It often happens that even first-class designs for girders are susceptible of modification when placed in the hands of the practical girder maker, not with a view to improvement from a theoretical point of view, but from that of practical expediency. When such works are in the hands of contractors resident in the same country as the engineer, these suggested modifications are easily explained and often accepted by the engineer. When, however, a set of girders is being made in England for an engineer, say, in India, it is often difficult to make any alteration in design or departure from the specification. Explanations take too long a time, and unless the modi-

fications offer very great advantages they are not made. Books of the more theoretical character seldom contain much that will guide the designer from other than theoretical considerations. Mr. Hutchinson's book takes up the other phase of girder engineering. Girders or bridges of certain size, span, and number being required, and the design from a theoretical side being selected, the book before us steps in with the information necessary to enable the engineer to decide upon the best method of carrying it out. It gives hints on practical expediency which lead to the selection of forms and sections of iron which may be incorporated into the structure without any loss of efficiency, often with considerable gain in this respect, and with economy in cost. Part I. is upon materials, and after giving a brief account of the rolling mill methods of producing iron of different forms, which is of much assistance in guiding the writer of the specification, it instructs the engineer on the question of relative cost of different sections. These are points of much importance, as an engineer may often specify sections difficult and costly to make, not perhaps because there is any reason why such sections should be used, but because he is ignorant of the practical considerations which make some methods of producing billets or slabs, or rolling certain sections much more expensive, without being any more efficient than others which the ironmaker or girder constructor can suggest.

Some useful hints are given on making up girder flanges composed of bar iron of various sections. The second part of the work is descriptive of examples of girder and bridge work, chiefly as carried out by the Skerne Ironworks Company. The particulars of a large number of bridges erected in this country and abroad are given, illustrated by many well executed engravings. The method of erection of some of these bridges is described, and these descriptions are very useful and interesting.—*Engineer.*

WATER ANALYSIS. By J. ALFRED WANKLYN. FIFTH EDITION. London: Trübner & Co.

The practical value of this work is sufficiently indicated by the fact that the present edition is the fifth.

The new matter is important, as it relates to the detection of the most important class of ingredients in impure water, a detailed account of the process of dealing with the organic matter being given for the first time in this edition.

A TREATISE ON METALLIFEROUS MINERALS AND MINING. By D. C. DAVIES, F.G.S. London: Crosby Lockwood & Co.

This is of a popular rather than a scientific character, and describes, in a sufficiently concise manner, the condition in which most of the metallic ores are found. The metals to which the bulk of the space is given, are gold, silver, copper, tin, lead, zinc and iron. Bismuth, Mercury and Nickel receive a brief notice.

Considerable space is given to the mechanical part of Mining Engineering, and in this part particularly the illustrations are good.

The book would be more widely useful, and possess quite as much interest to the general reader, if, instead of the geographical and historical notes, there were inserted a brief guide to the determination of the principal ores.

MANUEL DE L'INGENIEUR. DES PONTS ET CHAUSSEES. PAR A. DEBOUVE. Paris: Dunod.

This ponderous work fills eighteen volumes of text and twelve atlases of plates.

The list of subjects embraces everything that is conventionally assigned to the department of engineering, so that quite a complete practical engineer's library is represented by theseries.

The details of the public works in and about the chief French cities, are given with elaborateness and finish that is quite rare except in the best French treatises.

In all that relates to works in masonry; to the management of rivers, to drainage or to construction of roads, no better models can probably be found than those afforded by modern French engineering, and which are fully described in this comprehensive work of Debouve.

THE ART OF LETTER PAINTING MADE EASY. By JAMES BADENOCH. London; Crosby, Lockwood & Co.

This very brief book instructs the beginner in drawing letters by geometrical rules. The models given are not of the best, but the instruction is carefully detailed.

MISCELLANEOUS.

UPON the question of the abolition of blasting in coal mines and in favor of wedging, the mining engineers of North Staffordshire are contending that if wedging should be insisted upon by the Legislature to the sacrifice of blasting, then that certain of the thick seams would have to be abandoned, since holding in such measures was next to impossible. The statistics, it is urged, show that the loss of life is much greater from the effects of wedging and falls of the roofs and sides, than from blasting; and the North Staffordshire engineers hold that if blasting should be prohibited there will be greater danger from falls of roof.

TENDERS for the construction of the Adelaide sewers were sent in to the South Australian Government on June 16. The government intend using an area of 400 or 500 acres to utilize and pump the sewage, and this would be sufficient for effectually and efficiently disposing of the whole sewage of Adelaide and its suburbs.

THE new arsenal and dockyard to be founded at Mihara for the Japenese navy will include dry and wet docks fit for the largest war-ships; iron sheds, in which iron and wood war-vessels may be built in any weather, as well as foundries, engine-shops, rolling mills, stores, &c. The expense of these works, it is expected, will be very great, especially as there are to be barracks and fortifications for their protection.